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Fear without Prejudice in the Shadow of Jihadist Threat

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Abstract. Because the prejudice of the ingroup builds into fear of the outgroup, jihadist terrorism is expected to strengthen the politicized link between security and immigration. I use a causal inference in a clustered cross-country analysis to test the simultaneous short-run causal impact of the jihadist threat on security fear and ethnic prejudice of the public in Israel, the Netherlands, Russia, Sweden, France, and Germany. In line with common wisdom, jihadist attacks significantly increase security fear. Against it, jihadist attacks non-significantly decrease ethnic prejudice. This empirical pattern holds across different types of immigration attitudes, ethnic groups, intervals of time and terrorist events, and is robust to placebo treatments, placebo policy preferences, fake and failed terror attacks. These findings challenge extant consensus, and suggest that jihadist attacks, particularly at the local level, induce risk-aversion rather than desire for retaliation.

Keywords: Race, Ethnicity and Politics, Terrorism, Migration

1 Introduction

Following 9/11, scholars became increasingly interested in how security concerns, on top of cultural and economic ones (*e.g.*, Citrin, Green, Muste, & Wong, 1997,

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Mayda, 2006, Hainmueller & Hopkins, 2014, Valentino et al., 2017, Pardos-Prado & Xena, 2019), shape attitudes towards immigrants and minorities (*e.g.*, Lahav, 2010, Legewie, 2013, Messina, 2014, Böhmelt, Bove, & Nussio, 2019). Episodes of hate crimes (Hanes & Machin, 2014), labor (Davila & Mora, 2005), housing (Gautier, Siegmann, & Van Vuuren, 2009) or institutional discrimination (Shayo & Zussman, 2011) are thought to signal a worrisome shift in public attitudes (*e.g.*, Elsayed & De Grip, 2018). However, whether the public at large leans with or against such discriminatory behavior is still an open question.

This paper tests how jihadist attacks impact on the public's "security fear" and "ethnic prejudice". I propose a three-step empirical design. I begin by defining terrorist attacks as murderous plots perpetrated by members of known jihadist organizations and collect any attack that matches this definition from the global terrorism dataset (GTD, 2017). I then systematically check whether any such jihadist attack happened to have occurred during the fieldwork period of any round of the European Social Survey (ESS, 2016). Because the timing of the interviews is *as good as random* with respect to the date of each attack as they were scheduled earlier through strict random sampling, jihadist attacks represent plausibly exogenous variation of the level of security threat. I can hence use a causal inference in cross-country design to compare public attitudes before (the control group) and after (the treatment group) jihadist attacks.

Findings are as follows. In line with conventional wisdom, jihadist attacks significantly increase the public's security fear, capturing concern for safety. Against conventional wisdom, I find that jihadist attacks non-significantly decrease ethnic prejudice, encompassing all negative evaluations associated with immigrants and minorities. This empirical pattern holds across (i) different types of immigration attitudes, including those focusing on economic or cultural concerns, (ii) different ethnic groups, including the Muslim, Gypsies and Jewish migrants, (iii) varying time-bandwidths and (iv) diverse terrorist events. After conducting several robustness checks and placebo analyses, I conclude that the public reacts to terrorist attacks with *fear without prejudice*.

I interpret these findings borrowing from group-threat theory. Jihadist attacks may de-emphasize perceived *group* threat, which elicits desire for retaliation (see *e.g.*, Lahav & Courtemanche, 2012, Kam & Kinder, 2007), while emphasizing perceived *individual* threat, which elicits anxiety thereby lowering ethnocentrism (Lerner & Keltner, 2000, Fischhoff, Gonzalez, Small, & Lerner, 2003). Although this mechanism cannot be tested directly in non-experimental settings, conditioning attitudinal patterns on threat-exposure furthers its plausibility. Among more directly exposed respondents, who experience stronger security fear, the decrease in prejudice is more marked and less partisan (Huddy, Feldman, Taber, & Lahav, 2005, Huddy, Feldman, & Cassese, 2009).

From a methodological perspective, I contribute to the literature on terrorism and public opinion in two ways. Firstly, while previous research has largely focused on the transnational effect of jihadist attacks (Finseraas, Jakobsson, & Kotsadam, 2011, Legewie, 2013, Schüller, 2016, Nussio, Bove, & Steele, 2019), I focus on the effect of jihadist attacks at the local level, either national or regional. In doing so, I maximize compliance with the treatment, increasing the internal validity of the analysis. Secondly, while previous work restricts statistical inference to one case study only (Finseraas et al., 2011, Legewie, 2013, Schüller, 2016, Nussio et al., 2019, Van Hauwaert & Huber, 2020), I draw conclusions based on nine jihadist attacks that occurred between 2002 and 2016 in Israel, the Netherlands, Russia, Sweden, France, and Germany using the same empirical design. Such a design yields higher external validity.

These methodological differences are important beyond internal and external validity; they result in findings that substantively contrast with previous literature (Finseraas et al., 2011, Legewie, 2013, Schüller, 2016, Nussio et al., 2019, for an exception see Van Hauwaert & Huber, 2020). It is often assumed that since the prejudice of the ingroup builds into fear of the outgroup (*e.g.*, Quillian, 1995), stronger outgroup-driven security threats should monotonically increase ethnic prejudice (Legewie, 2013). Seen as the behavioral counterpart of a widespread attitudinal shift, episodes of discrimination against Muslims in the shadow of jihadist threat have reinforced this view (*e.g.*, Hanes & Machin, 2014). My findings indicate that this conventional wisdom should be requalified: the correlation between security fear and ethnic prejudice, relevant in peaceful times, vanishes precisely under the most salient of all threats. Terrorism and the fear of it thereof may thus entail an ironically positive effect on democracy because heightened risk-aversion fosters information-seeking and reduces prejudice; "a worried citizen is a good citizen" (Valentino, Hutchings, Banks, & Davis, 2008).

The paper proceeds as follows. Firstly, I bridge the literature on group threat theory with that on terrorism and public opinion to set out expectations (section 2). Then I present the empirical design (3). In doing so, I place emphasis on the various steps that lead from the definition of terror attacks to the collection of episodes, and from the collection of episodes to the estimation strategy (3.1). I also discuss the main threats to identification (3.2). Section 4 presents and discusses the main results at several levels of aggregation. Finally, section 5 highlights strengths and weaknesses of the paper, discussing avenues for future research.

2 Literature and hypotheses

Terrorism is generally conceptualized as requiring adjustments in the balancing of civil liberty against national security (*e.g.*, Waldron, 2003). However, counterterror policies often trade-off a flagrant reduction of civil liberties against an uncertain increase in national security (*e.g.*, Dragu, 2011). Immigration policy makes no exception. On the one hand, an immigration ban on ethnic minorities may halt the jihadist network thereby *ceteris paribus* lowering long-run terrorist activity.[1] On the other hand, explicitly targeting ethnic minorities may initiate a process of risk subjectification that alienates minorities (Mythen, Walklate, & Khan, 2013), potentially aligning them with the terrorist organization (Ingram, 2019) and ultimately aggravating those political grievances that contributed to the emergence of terrorism in first place (Walsh & Piazza, 2010, Bueno de Mesquita & Dickson, 2007).

In this context, the public is both decisive and uncertain. It is decisive because it expedites or hinders the process by which immigration rules are decided "out of the realms of conventional policy-making and into the domain of emergency politics" (Messina, 2014, p532). It is uncertain because it is exposed to a threat that activates conflicting feelings and coping mechanisms, possibly resulting in opposite attitudinal responses.

2.1 The public under terror threat

Group-threat theory is the single most important framework to study the attitudes of citizens, the "ingroup", towards immigrants and minorities, the "outgroup" (for a review, see Hainmueller & Hopkins, 2014). That individuals value their own membership group over groups to which they do not belong enjoys a nearly axiomatic status (*e.g.*, Brewer, 2007). It is equally well-established that, in a world characterized by scarce resources, the ingroup perceives the outgroup as a threat to its privilege (Quillian, 1995).

While postulating that terrorist attacks affect perceived threat is relative uncontentious (Legewie, 2013), the mapping between perceived threat and attitudinal patterns is far from trivial. It is characterized by (i) the relevant target of the threat and associated psychological reactions (Lerner & Keltner, 2000, Huddy et al., 2005);

(ii) the relevant dimension of the threat, possibly encompassing security, economic, or cultural concerns (Canetti-Nisim, Ariely, & Halperin, 2008, Lahav & Courtemanche, 2012, Ben-Nun Bloom, Arikan, & Lahav, 2015); and (iii) the degree of homogeneity of the outgroup (Pickett & Brewer, 2001, Bar-Tal & Labin, 2001).

Group vs individual threat. The most important distinction is that between *group* threat and *individual* threat (*e.g.*, Stephan & Stephan, 2013). The former is perceived to target the whole group to which one belongs to without directly targeting the individual, whereas the latter is perceived to directly target the single individual and her close ties (Rosenstein, 2008). Different target-perceptions result in different psychological reactions (Cottrell & Neuberg, 2005), leading to opposite risk-profiles (Fischhoff et al., 2003, Joslyn & Haider-Markel, 2018). On the one hand, group threat produces anger, leading to *under*-estimates of the risk of intergroup conflict. On the other hand, individual threat produces anxiety, leading to *over*-estimates of the risk of intergroup conflict.

In turn, opposite risk-profiles predict opposite attitudinal adjustments (Fischhoff et al., 2003, Huddy et al., 2005, Huddy et al., 2009). Under-estimating the risk of intergroup conflict makes individuals comfortable with group-antagonism, including *e.g.* authoritarianism and exclusionist political attitudes (Canetti-Nisim, Halperin, Sharvit, & Hobfoll, 2009, Hetherington & Suhay, 2011), negative immigration attitudes (Lahav & Courtemanche, 2012, Legewie, 2013), ethnocentrism (Kam & Kinder, 2007, Brader, Valentino, & Suhay, 2008) and support for punitive actions against the outgroup (Bar-Tal & Labin, 2001). By stark contrast, over-estimating the risk of intergroup conflict leads individuals to adopt safe, conciliatory attitudes towards the outgroup (Huddy et al., 2005).

The distinction between country and individual threat yields two competing hypotheses. If group threat and desire for retaliation dominate, terrorist attacks should increase ethnic prejudice along with security fear. If individual threat and anxiety dominate, terrorist attacks should successfully spread security fear without increasing ethnic prejudice.

While disentangling the group and individual component of perceived threat is difficult in non-experimental settings, there exist two indirect, complementary ways to test the consistency of the proposed mechanism. Firstly, we should expect more directly targeted individuals to experience stronger anxiety under terror threat. Following 9/11, residents in the New York metropolitan area displayed substantially higher anxiety relative to residents in the North-East. This led them to oppose risky policies that would have retaliated against Arab communities (Huddy et al., 2005). Secondly, we should expect more anxious individuals to forgo ideological processing. Following the *Orlando shooting,* anxious liberals and anxious conservatives aligned on key political and policy attitudes related with mass-shooting (Joslyn & Haider-Markel, 2018).

To summarize, the aggregate effect of jihadist threat on ethnic prejudice is ambiguous. Jihadist threat is more likely to increase ethnic prejudice and to ideologically polarize among weakly exposed individuals.[2] Instead, focusing on the local effect of jihadist attacks should lead to nonpartisan, risk-averse public opinion responses.

Realistic vs symbolic threat. Whether it targets the group or the individual, threat is multidimensional; it is *realistic* if it harms physical safety or wealth and *symbolic* if it harms the ingroup's cultural identity (Riek, Mania, & Gaertner, 2006). Canetti-Nisim et al. (2008) argue that security threats affect attitudes towards ethnic minorities more directly than economic or cultural threats do (Canetti-Nisim et al., 2008). Assuming that jihadist terrorism represents a security threat, we should expect it to affect those immigration attitudes that capture ethnocentrism, rather than those that proxy cultural or economic concerns (Kam & Kinder, 2007).

However, jihadist terrorism also affects the economy (*e.g.*, Dixon, Rimmer, Wittwer, Rose, & Heatwole, 2017), informs about changes in power relationships among nations (*e.g.*, Findley, Piazza, & Young, 2012), and heightens both social cleavages, by reducing trust (Geys & Qari, 2017a), and cultural cleavages, by making stereotypes salient (*e.g.,* Obaidi, Kunst, Kteily, Thomsen, & Sidanius, 2018). The multifaceted nature of terrorism as a social phenomenon, together with the strong correlation among different types of immigration attitudes (see *e.g.*, Ben-Nun Bloom et al., 2015), mean that jihadist attacks may ultimately affect different immigration attitudes in a similar way (*e.g.*, Finseraas et al., 2011, Legewie, 2013).

Following the idea that terrorism is mainly a security threat, I focus on those immigration attitudes that more directly account for ethnic prejudice. However, acknowledging that threat is multidimensional, I also consider those immigration attitudes that weigh economic or cultural concerns, expecting them to evolve with a similar dynamic.

Homogeneous vs differentiated outgroup. The threatening outgroup can be perceived as *homogeneous* or *specific*. Intuitively, attitudinal changes should be limited to attitudes towards Muslims in the context of jihadist terrorism. Three related observations cast doubt on such intuition. The construction of social identity reflects both the desire to assimilate with an ingroup and the desire to differentiate with respect to an outgroup (Brewer, 1991). In turn, stressing intragroup similarities accommodates the desire to assimilate, whereas stressing social distance with respect to the outgroup accommodates the desire for distinctiveness (Rothgerber, 1997). And since intergroup threat broadens the scope for group cohesion, it also strengthens desire for both assimilation and distinctiveness, leading to the refining of ingroup membership and, correspondingly, to the perception of greater outgroup homogeneity (Pickett & Brewer, 2001).

The empirical literature backs perceived group homogeneity in several contexts. The seminal contribution of Bar-Tal & Labin, 2001 provides evidence for the effect of threat on perceived group homogeneity within the context of terrorism in Israel. Bar-Tal & Labin, 2001 find that during peaceful times, Israeli teenagers generally hold a specific ethnoracial hierarchy of groups, based on threat levels, placing Jordanians above Arabs, and Arabs above Palestinians. However, when asked to complete the same survey following a series of terrorist attacks carried upon by the Palestinians, their ethnic prejudice increases similarly across different groups. Focusing on the case of Spain, Echebarria-Echabe & Fernandez-Guede, 2006 reinforce this result. They show that the jihadist attack in Madrid on March 11, 2004 increased anti-Arab and anti-Semitic prejudice in a similar manner. Such a homogenization effect may be furthered by the parallel effect of threat on authoritarianism (Canetti-Nisim et al., 2008). As put by Joshua Legewie, "Islamic terrorism precipitated a debate about immigrants and immigration in general" (Legewie, 2013, p1202). Consistently, the empirical work in the literature of terrorism and public opinion to which I connect to does not make a distinction between Muslims and other groups (Finseraas et al., 2011, Schüller, 2016, Nussio et al., 2019).

Considering existing theory and evidence, I expect the ingroup to perceive greater outgroup homogeneity. Changes in ethnic prejudice should be thus be similar across different ethnic groups. I test for outgroup homogeneity by comparing the dynamic of ethnic prejudice against ethnic Muslims, Gypsies, and Jews.

3 Empirical analysis

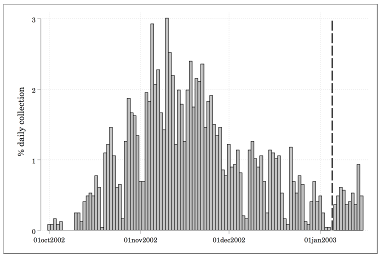
The *GTD* defines terror(ist) attacks as "the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion or intimidation". I confine the focus on (i) terrorist plots

(ii) where at least one person is killed (iii) carried out by members of jihadist organizations. Each building block of the proposed definition fulfills desirable identification features. I focus on (i) terrorist plots to distinguish actual terrorist attacks from episodes of crime perpetrated by Muslims. I focus on (ii) murderous attacks to distinguish the major security threats from minor ones. Finally, I focus on (iii) terrorist attacks perpetrated by affiliated members to restrict attention to international terrorism. The appendix provides detailed examples of jihadist attacks that match each of the building blocks of my definition, as well as examples of episodes that do not qualify as jihadist attacks given my definition.[3]

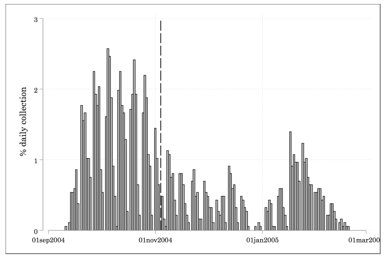
I systematically check whether each such jihadist attack recorded in the Global Terrorism Dataset (GTD), which globally includes about $170,000$ terrorist attacks, happened to have coincided with the survey fieldwork period of the targeted country scheduled by the European Social survey (ESS), which includes about 25 countries in eight waves ranging from 2002 to 2016. Importantly, given the focus on the local rather than transnational terrorism, I only retain those jihadist attacks perpetrated in a particular country that occurred during the fieldwork period of that same country. By doing so, I am able to collect nine jihadist attacks of heterogeneous magnitude that occurred in Israel, the Netherlands, Russia, Sweden, France and Germany between 2003 and 2016.

These attacks are briefly summarized in the caption of Figure 1, which plots the daily distribution of survey collection around each attack. In Figure 1, the gray spikes give the density of the daily survey collection whereas the black dashed spike is the date at which each attack took place. Individuals interviewed before any attack are in the control group. Vice-versa, individuals interviewed after any attack are in the treatment group.

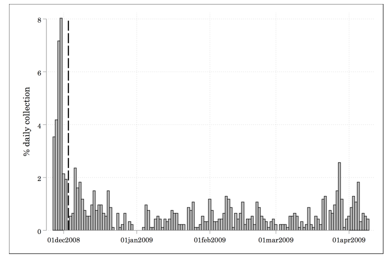
FIGURE 1: DATA COLLECTION AROUND TERRORIST ATTACKS.



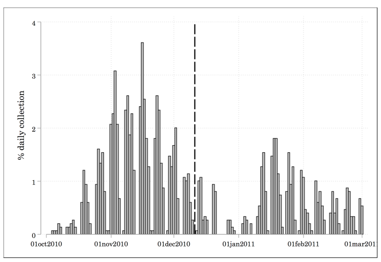
(A) **Israel, 2003.** On January 5, two suicide bombers from *Al-Aqsa Martyrs Brigade* killed 22 in Tel Aviv. Deadliest attack in Israel since March 2002.



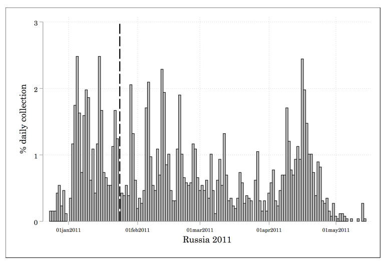
(B) **Netherlands, 2004.** On November 2, a member of the ductch Islamist group *Hofstad network* killed the Dutch film maker Theo Van Gogh in Amsterdam.



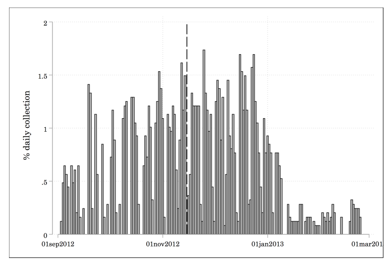
(C) **Russia, 2008.** On December 3, members of *Caucasus Emirate* fired upon civilians, killing three in Agishty, Chechnya.



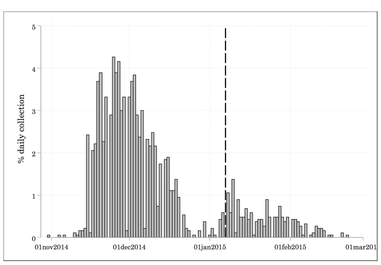
(D) **Sweden, 2010.** On December 11, a member of *Al Quaeda* set two bombs in central Stockholm, killing himself and injuring two. Thought as the first suicide attack in the Nordic countries linked to Islamic terrorism.



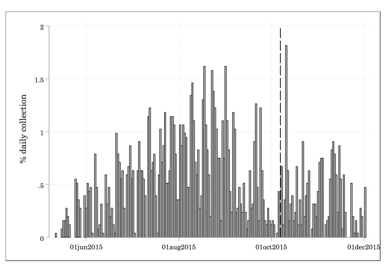
(E) **Russia, 2011.** On January 24, a suicide bomber from *Caucasus Emirate* killed 38 at Moscow airport. Deadliest terrorist attack in Russia since November 2004.



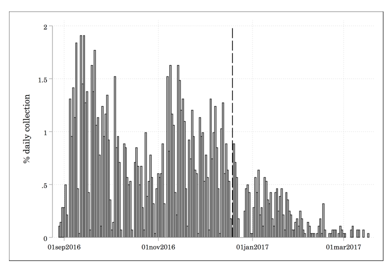
(F) **Israel, 2012.** On November 15, a member of *Hamas* killed three civilians in Kiryat Malachi city.



(G) **France, 2015.** On January 7, two members of *Al Quaeda* killed 12 at the headquarters of *Charlie Hebdo,* Paris. The day after, one policeman and four Jews are also killed. Third deadliest attack in France since at least 1972.



(H) **Israel, 2015.** On October 7, a member of *Hamas* stabbed an Israel Defense Forces (IDF) soldier in Kiryat Gat city and is killed.



(I) **Germany, 2016.** On December 19, a member of *Isis* drove a truck into a Christmas market in Breithscheidplatz killing 12 people, Berlin.

Notes. For each attack, the grey spikes give the density of the daily survey collection. The black dashed spike is the date at which each attack took place. Units interviewed in the same day as the attack are deleted. Source: European Social Survey (ESS), rounds 1 to 8, and the Global Terrorism Dataset (GTD), from which the descriptions are taken.

3.1 Data and estimation

Individual-level data come from eight rounds of the ESS. The survey was constructed using strict random probability sampling. In the ESS, an interview is conducted face to face and usually lasts for about one hour. The dataset combines detailed information about the socioeconomic status of the respondents with a variety of items on political orientations. The ESS sampling guidelines establish that random probability methods must be chosen at each stage and that quota sampling or substitution of the non-respondents is not permitted at any point in time. Further details regarding the survey design are discussed in the appendix (A.2.1).

I begin by testing whether jihadist attacks cause the public to jointly increase security fear and ethnic prejudice. To proxy the *security fear*, I use the following item:

- "It is important that the government ensures safety against all threats. The state must be strong so it can defend its citizens." (0) "Completely disagree" to (5) "Completely agree";

Sharp changes in this survey item should reflect the effectiveness of jihadist attacks in spreading fear. In appendix, I consider an alternative item that does not refer to government intervention (see A.4.2).

To proxy the *ethnic prejudice*, I use the following items:

- To what extent do you think your country should allow people of the *different* race or ethnic group as most country's people to come and live here? 1: "Allow many" to 4: "Allow none";

- To what extent do you think your country should allow people of the *same* race or ethnic group as most country's people to come and live here? 1: "Allow many" to 4: "Allow none".

The succession of the two questions explicitly primes the role of race/ethnicity, and the two questions only differ in the race dimension. I generate the variable ethnic prejudice as follows:

Oppose migrants of *different* ethnicity – Oppose migrants of *same* ethnicity.

While traditional definitions of "prejudice" emphasize the preconceived nature of negative attitudes (Allport, Clark, & Pettigrew, 1954), recent conceptualizations incorporate functional and experiential aspects of prejudice (Dovidio, Schellhaas, & Pearson, 2019) to encompass all negative emotion or evaluation associated with immigrants and minorities (Stephan & Stephan, 2013).

Hypothesis testing yields four possible scenarios:

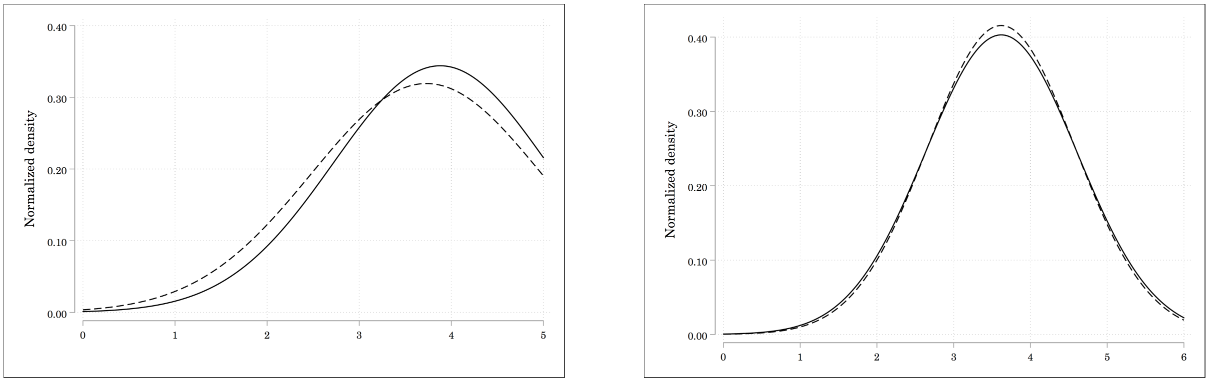
- *Fear with prejudice*: Jihadist attacks increase security fear and ethnic prejudice;
- *Prejudice without fear:* Jihadist attacks increase ethnic prejudice only;
- *Fear without prejudice*: Jihadist attacks increase security fear only;
- Terrorist attacks are ineffective.

Figure 2 provides the normalized distribution of the two dependent variables for the control and treatment group. The left panel of Figure 2 shows that security fear has higher mean and lower standard deviation after terrorist attacks. The right panel of Figure 2 shows that ethnic prejudice has lower mean higher standard deviation after terrorist attacks.

I estimate the following equation:

$$y_{i,g} = \alpha + \beta T_{i,g} + \gamma' \mathbf{x_{i,g}} + \theta_g + \mu_{i,g}.$$

FIGURE 2: DISTRIBUTION OF THE DEPENDENT VARIABLES.



(A) **Security fear.** Before terrorist attacks, the mean is 3.733 and the standard deviation is 1.251 (10,407 observations). After the attack, the mean is 3.882 and the standard deviation is 1.162 (6,387 observations).

(B) **Ethnic prejudice.** Before terrorist attacks, the mean is 3.620 and the standard deviation is .962 (10,342 observations). After the attack, the mean is 3.673 and the standard deviation is 1.020 (6,151 observations).

Notes. Normalized distribution of the two dependent variables before (regular line) and after (dashed line) terrorist attacks. Source: ESS.

$y_{i,g}$ is the score in each dependent variable for unit i is in country-year g. α is the intercept whereas β is the "terror treatment effect". $T_{i,g}$ is the treatment indicator. It takes value 1 if the unit was interviewed after the terrorist attack and 0 otherwise. The choice of time bandwidths is necessarily arbitrary. A short bandwidth maximizes the plausibility of attributing of the observed pattern to the terrorist event. A longer bandwidth, however, permits to gain statistical power. I balance out attribution and statistical power using a 15 days bandwidth for the main analysis, providing sensitivity analysis in section 3.2.

The empirical specification includes a vector of baseline individual covariates, $\mathrm{x_{i,g}}$. The set of sociodemographic control variables include income decile ($1-10$), education attainment ($1-7$), gender ($0-1$), age ($15-99$) and squared age, household status (children living at home *vs.* no child living at home), immigration background (either the individual, his or her mother or father or both born in a different country), the employment status (employed *vs.* unemployed), and domicile fixed effects (where

0 represents big city and 4 represents the rural countryside). θ_g is a country-year specific intercept and $\mu_{i,g}$ is the error term.

To ease the interpretation of the terror treatment effects, the model is fit through OLS. Non-linear models may provide a better fit to the data generating process, and they are considered in appendix (A.3.3). Finally, I use robust standard errors and discuss some clustering strategies in appendix (A.3.4, A.3.5).

3.2 Threats to identification

The empirical design proposed here is subject to three major threats to identification that I briefly discuss and extensively cover in the appendix.[4]

Conditional ignorability: The respondents' treatment status should be independent of their potential outcomes, which are conditional on a set of covariates. Two major threats to conditional ignorability are addressed: (i) imbalance and (ii) attrition. (i) The actual treatment assignment reflects a set of sampling decisions, possibly resulting in an imbalance between the control and treatment groups. For instance, the easier it is to reach a respondent, the more likely that she or he is in the control group (Legewie, 2013). I address both the imbalance in the covariates and in geographic units (A.2.1). If the regions were sampled at different points in time, the outcomes could be biased by a regional imbalance.[5] I deal with imbalance in the covariates and geographic units by combining entropy balancing (Hainmueller, 2012) with outliers' deletion through pretreatment coarsened exact matching (King, Blackwell, Iacus, & Porro, 2010, Iacus, King, & Porro, 2012, Bol & Giani, 2019). The details are discussed in appendix (A.2.1). (ii) There are two potential attrition issues. First, individuals may become unwilling (or more willing) to take the survey because of terrorist attacks. To rule out potential drops in survey collection in correspondence with terrorist attacks, I compare that survey collection rates in terror-targeted regions - where the strongest drop should be observed if attrition was a problematic issue - against all other regions before and after terrorist attacks (A.2.2). I show that the survey collection rates in areas targeted by terrorist attacks stay constant in the immediate aftermath of the attacks. Second, individuals may become unwilling (or more willing) to give a valid answer to either dependent variable because of terrorist attacks. Further analysis rules out this possibility (A.2.3).

Excludability: No other event than the terrorist attack should affect the outcome. Two major threats to excludability are addressed: (i) unobserved time-varying trends and (ii) compound treatment. (i) The observed terror treatment effects may spuriously reflect unobserved time-varying trends. It appears reasonable to choose time bandwidths that are short enough to minimize attribution issues and large enough to achieve sufficient statistical power. I assess the robustness of the main outcomes to varying time bandwidths as well as proposing an augmented specification in which we increase the number of control variables by including a linear and a quadratic time trend. In addition, we show that running permutation tests shows that resampling the treatment variable within each country–year systematically results in null simulated treatment effects (A.2.4). (ii) The terror treatment effects may weigh the collateral events. For instance, terrorist attacks trigger political reactions that align with attitude formation. This possibility cannot be ruled out. What can be ruled out, instead, is that the reported evolution of ethnic prejudice reflects major changes in immigration-related attitudes that are simultaneously taking place in the sampled countries for reasons unrelated to terrorism. The appendix reports the treatment effects for a set of additional survey items that act either as alternative dependent variables for security fear (A.4.1), or as placebos for it (A.4.2).

Full compliance: For the assignment to the treatment group to be valid, individuals must be informed about the occurrence of terrorist events. Conversely, individuals in the treatment group who are unaware of the attack should have been assigned to the control group. Noncompliance would bias the treatment effects downward, making false positives less likely. Because the assumption of full compliance cannot be directly tested, it may be more appropriate to label the treatment *intention to treat*. Yet two different arguments render plausibility to the assumption of full compliance. First, my focus on local terrorism makes full compliance more credible because individuals are more likely to be informed about a terrorist attack if the latter happened in their country. A similar argument applies when it comes to my focus on murderous jihadist attacks, which are presumably more salient than the fake and failed attacks analyzed in the appendix (A.5). Second, an analysis of Google trends during the fieldwork period of each attack confirms that the searches for "terrorism" are the highest on the day of the attacks (A.6).

4 Fear without Prejudice

The first four columns of Table 1 show that terrorism increases *security fear*. The outcomes are not model dependent: adding socioeconomic covariates (models (ii) and (iii)) or extracting the outliers through pretreatment matching (model (iv)) results in minor changes. In the full specification, being interviewed after terrorist attacks increases security fear by about three percentage points. Because ensuring security is widely considered a primary function of the state, the mean of security fear is rather high (≈ 3.8, see Figure 2), with 32% of sampled individuals reporting maximum score. Ceiling effects may hence mitigate the magnitude of the terror treatment effect. In Table 1, we observe that terrorist attacks cause a slight downward shift in *ethnic prejudice*. According to each model specification, with minor differences along columns (i) to (iv), the terror treatment effect is negative, yet the effect is mild, and the standard errors are relatively large. The public reacts to jihadist attacks with fear without prejudice.

TABLE 1: FEAR WITHOUT PREJUDICE.

	Security fear				Ethnic prejudice			
	(i)	(ii)	(iii)	(iv)	(i)	(ii)	(iii)	(iv)
Treatment	.132	.133	.159	.160	-.033	-.041	-.038	-.037
	(.032)	(.032)	(.036)	(.036)	(.026)	(.026)	(.028)	(.028)
Country FE	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Domicile FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Matching	No	No	No	Yes	No	No	No	Yes
N.obs	11,981	11,976	10,019	9,881	11,878	11,870	9,900	9,762
R-squared	.14	.15	.17	.17	.19	.20	.20	.20

Notes. The reported coefficients are treatment effects on security fear and ethnic prejudice estimated by OLS. Robust standard errors (in parentheses) are clustered at individual level. In each regression, the control group is weighted using entropy balancing. Through the latter, the covariates' distribution in the control group mimics the first and second moment of the equivalent distribution in the treatment group. I balance the control and treatment group according to the same variables used as controls in each specification. Each specification includes country-year fixed effects. Domicile fixed effects account for the level of urbanization of the household. The treatment variable is a dummy taking the value 1 if the respondent was interviewed after each of the recorded jihadist attacks, during an interval of 15 days. Controls include income decile (1-10), education (1-7), age (15-99), gender (0-1), household status (0-1), migration background (0-1) and employment status (0-1). Outliers' extraction is carried through Coarsened Exact Matching. Income is coarsened around the median category (5), education is coarsened at university degree (5), age is coarsened in intervals of 10 years within the working age following the application of Blackwell et al. (2009). For each unbalanced dummy variable I apply exact matching. Data include all nine terror attacks as in Figure 1. Source: ESS, rounds 1-8.

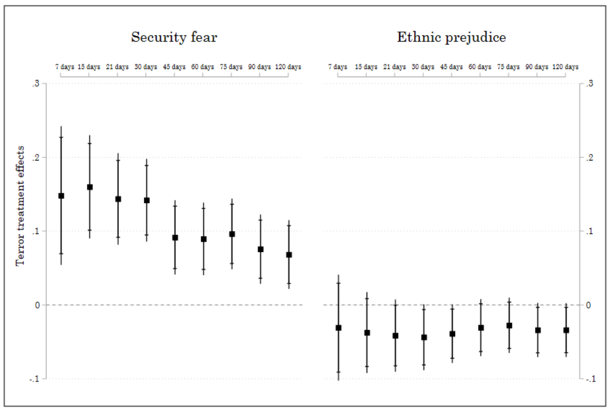
The empirical pattern holds true across different time horizons and terrorist incidents. Although the analysis presented in Table 1 focus on the very short term (within 15 days after each attack), Figure 3a summarizes the terror treatment effects for alternative time bandwidths. The terror treatment effects are qualitatively and quantitatively similar across alternative intervals of time, either shorter or longer. The left panel of Figure 3a focuses on security fear. It shows that the treatment is the strongest after one week. They it slightly decays. The right panel of Figure 3a focuses on ethnic prejudice. It shows that the latter softens to a similar extent across different bandwidths.

Figure 3b shows that the aggregate increase in security fear is driven by some of the largest attacks, including the attack perpetrated by *Hamas* in Tel Aviv in 2003 (22 casualties), the *Domodedovo Airport shooting* in Moscow in 2008 (38 casualties), and the *Charlie Hebdo shooting* in Paris in 2015 (17 casualties). The terror treatment effect on security fear is also large in one of the smaller scale attacks in the sample - the attack perpetrated in the Netherlands in 2004 against the Dutch film director and intellectual Theo Van Gogh. Interestingly, Figure 3b shows that the aggregate decrease in ethnic prejudice is also driven by some of the largest attacks, including the attack in Tel Aviv in 2003 (22 casualties), the *Charlie Hebdo shooting* in 2015 (17 casualties), and the *Berlin truck attack* in 2016 (12 casualties). Disaggregate findings indicate that the main terror treatment effects are not driven by a single outlier, and the heterogeneity at the terror episode level is overall bounded. However, such analysis suffers from low statistical power, hence the terror treatment effects should be taken with caution.

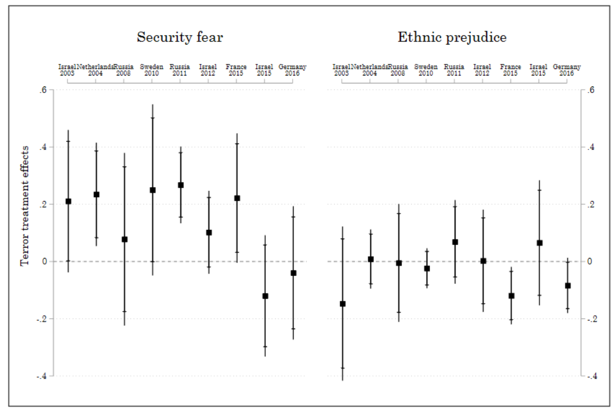
Group vs individual threat. The empirical analysis discloses an empirical puzzle. Although security fear and ethnic prejudice are indeed correlated in the control group ($\rho_{\text{before}} = .11$), such correlation drops by 75% ($\rho_{\text{after}} = .03$) precisely when citizens are

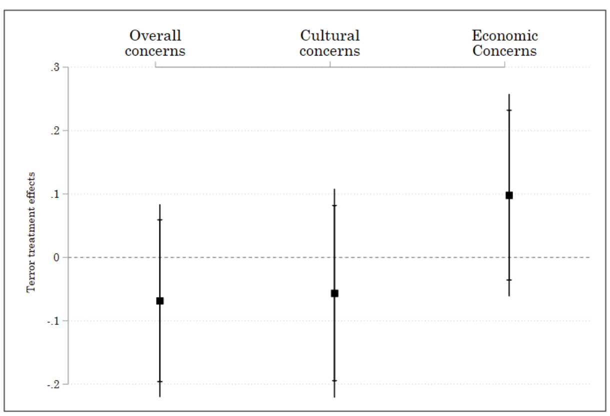
FIGURE 3: TERROR TREATMENT EFFECTS ACROSS TIME, TERRORIST EPISODE, IMMIGRATION CONCERN AND ETHNIC GROUP.

(A) By time bandwidths.

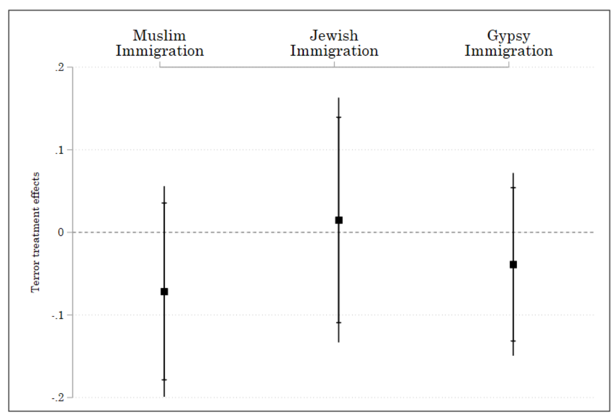


(B) By terrorist episodes.





(C) By immigration concern.



(D) By ethnic group.

Notes. Each figure plots the terror treatment effects obtained through the same model specification as in Table 1, adding .95 (whole plot) and .90 (capped plot) confidence intervals. Details about regressions coefficients, standard errors and number of observations are reported in appendix. Source: ESS.

exposed to the most salient security threats. What could explain such an empirical pattern? I argued in section 2.1 that if jihadist attacks are mainly perceived as a threat to the country, then the resulting threat-perception increases desire for retaliation and worsens ethnic prejudice. Instead, if jihadist attacks are mainly perceived as a threat to one's personal safety, then the resulting threat-perception induces anxiety and risk-aversion, softening ethnic prejudice. Thus, a possible interpretation of my outcomes is that, in the short-run, jihadist attacks increase perceived individual threat more than they increase perceived country threat. I now provide indirect evidence consistent with the proposed mechanism in two complementary ways.

Firstly, like residents in the New York metropolitan area following 9/11 (Huddy et al., 2005), individuals living in the area targeted by terror attacks should perceive the strongest anxiety and thus soften their ethnic prejudice to a greater extent than individuals living elsewhere do. Table 2 reports the coefficients for an additional specification in which I split the sample in two depending on whether or not the respondent lives in the area targeted by the terrorist attack.[6] The coefficients in Table 2 strongly supports this possibility: security fear increases and ethnic prejudice decreases to a substantially greater extent among respondents living in the area targeted by terror attacks. The aggregate terror treatment effects in Table 1 are indeed driven by individuals with higher exposure to the terror threat.

Secondly, anxiety leads citizens to alleviate ideological processing, making public opinion responses similar across ideological groups that hold otherwise different stances (see *e.g.*, Joslyn & Haider-Markel, 2018). Since such a phenomenon is typical of anxiety, we should expect ideology to unify the public in targeted regions and to polarize it in others. It should hold that ideology plays no moderating effect in target regions while potentially polarizing in other regions. The coefficients in Table 2, back such a mechanism. I interact the terror treatment with a dummy variable that takes the value of one if the respondent self-reports leftwing (less than 5) on the traditional 1-10 scale. The interaction effect is positive and non-significant in the target region, indicating that leftwing individuals, who hold on average lower ethnic prejudice, react to jihadist attacks in a very similar manner to all other individuals (fourth column). Instead, the interaction effect is negative and significant in other regions (second column), indicating that ideology does play a role when individuals are less concerned about their personal safety.

TABLE 2: ANXIETY AND RISK AVERSION.

	All regions		Target region	
	Fear	Prejudice	Fear	Prejudice
Treatment	0.16	0.01	0.53	-0.16
	(0.05)	(0.04)	(0.16)	(0.09)
Treatment $\times$ Leftwing	-0.07	-0.14	-0.32	0.22
	(0.10)	(0.06)	(0.32)	(0.18)
Country FE	Yes	Yes	Yes	Yes
Domicile FE	Yes	Yes	Yes	Yes
Matching	Yes	Yes	Yes	Yes
N.obs	6,895	6,782	548	542
R-squared	0.16	0.16	0.37	0.19

Notes. The reported coefficients are treatment effects on security fear and ethnic prejudice estimated by OLS. Robust standard errors (in parentheses) are clustered at individual level. In each regression, the control group is weighted using entropy balancing. Through the latter, the covariates' distribution in the control group mimics the first and second moment of the equivalent distribution in the treatment group. I balance the control and treatment group according to the same variables used as controls in each specification. In this case, however, I also weight and control for regions, which corresponds to NUT2 (NUT3 for the Netherlands). Each specification includes country-year fixed effects. Domicile fixed effects account for the level of urbanization of the household. The treatment variable is a dummy taking the value 1 if the respondent was interviewed after each of the recorded jihadist attacks, during an interval of 15 days. Controls include income decile (1-10), education (1-7), age (15-99), gender (0-1), household status (0-1), migration background (0-1) and employment status (0-1). Outliers' extraction is carried through Coarsened Exact Matching. Income is coarsened around the median category (5), education is coarsened at university degree (5), age is coarsened in intervals of 10 years within the working age. For each unbalanced dummy variable I apply exact matching. Data include six out of nine terror attacks in Figure 1. Israeli data drop since I do not have information about the region where respondents live. Source: ESS, rounds 1-8.

Realistic vs symbolic threat. In section 2.1, I explain that since jihadist terrorism is not uniquely a security threat, but also an economic and cultural one, it is reasonable to expect that all immigration attitudes evolve in a similar way. I proxy further immigration-related concerns as follows:

- Overall concerns: Immigrants make country worse or better place to live. 1: Worse; ... ; 10: Better.
- Cultural concerns: Country's cultural life undermined or enriched by immigrants. 1: Undermined; ... ; 10: Enriched.
- Economic concerns: Immigration bad or good for country's economy. 1: Bad; ... ; 10: Good.

Figure 4c confirms that the terror treatment effects on these standard immigration proxies are null, and follow a similar pattern as those presented in Table 1.

Homogenous vs differentiated outgroup. Previous research has shown that jihadist terrorism is expected to foster discriminatory attitudes not only toward Muslims, but also toward other ethnic minorities (*e.g.* Canetti-Nisim et al., 2009). Hopefully, the seventh round of the ESS includes more detailed information about specific ethnic groups. The survey items are the following:

- Gypsy immigration. Allow many or few Gypsies to come and live in country. 1: Many; ... ; 4: None.
- Jewish immigration. Allow many or few Jews to come and live in country. 1: Many; ... ; 4: None.
- Muslim immigration. Allow many or few Muslims to come and live in country. 1: Many; ... ; 4: None.

Figure 4d shows that the treatment effect on ethnic prejudice performs similarly across different ethnic groups. This confirms that security threats lead the public to undifferentiated responses.

5 Conclusion

Governments around the globe did not necessarily make concessions to terrorist groups (Abrahms, 2012), but sometimes responded to 9/11 by suspending civil liberties (*e.g.,* Epifanio, 2011). Whether this social cost was worth it in terms of higher security is theoretically contestable and empirically unclear (*e.g.,* Waldron, 2003, Daxecker & Hess, 2013). In this context of policy uncertainty, public opinion is pivotal.

In the current paper, I test whether jihadist attacks jointly increase security fear and ethnic prejudice. I propose an empirical protocol that exploits the timing of survey interviews, which is random regarding the exact date of the terrorist attacks, to analyze nine terrorist attacks in six countries between 2002 and 2016. My analysis confirms that terrorism, particularly in the targeted terror areas and in relation to large-scale terrorist incidents, spreads fear. This effect survives in the medium run; terrorism calls for policy responses. Yet within diverse contexts, the public does not give in to ethnic prejudice towards immigrants and minorities.

These findings offer an important contribution to the empirical literature on terrorism and public opinion. Largely based on transnational studies and single-case studies, the extant consensus is that jihadist attacks worsen attitudes towards immigrants and minorities. Such a consensus should be updated since it does not hold at more disaggregated geographic levels, whatever time-bandwidth, terrorist episode, type of immigration attitude and ethnic group one chooses. I argue that the distinction between (perceived) group and individual threat offers a theoretical basis for the disclosed attitudinal pattern.

Some caveats are in order. While the focus on immigration attitudes is important, it would be interesting to single out other policy areas that the public link to security concerns. Support for hawkish foreign policy or extrajudicial practices such as arbitrary detention or torture may increase following terrorist attacks. Unfortunately, the *ESS* does not include information about these policies. Moreover, my analysis abstracts from the role of leaders' rhetoric in mitigating the treatment effects. Leaders' reaction speeches and their media coverage may be exploited to enhance our understanding of public responses, allowing for the testing of further hypotheses. Finally, this paper's sole focus on the individual level does not allow us to grasp the effects of institutional and contextual factors that previous research has shown to be key in moderating ethnic prejudice within an ingroup-outgroup divide (*e.g.*, Quillian, 1995). Multi-level modeling may complement existing approaches and provide new directions in the field.

NOTES

[1]Bandyopadhyay & Sandler (2014) show that, from the perspective of a developed country, immigration rules that limit unskilled labor from developing countries successfully reduce the risk of terrorist attacks. This is partially supported by the empirical literature. Bove & Böhmelt (2016) use a series of spatial temporal autoregressive models and focus on 145 countries for 30 years to study how immigration flows affect the likelihood of terrorist events. They show that appropriate restrictions based on the countries of migrants' origins may help restoring homeland security. Further empirical research confirms that ethnic polarization, particularly when interacted with urban concentration of dangerous individuals (Ezcurra, 2017), increases the likelihood of terrorist activities (Danzell, Yeh, & Pfannenstiel, 2016).

[2]Consistently, extant quasi-experimental evidence based on transnational studies find that jihadist attacks worsen attitudes towards immigrants and minorities. Finseraas et al. (2011) show that the murder of film director Theo van Gogh in Amsterdam, Netherlands on October 2, 2004 increased support for restrictive immigration policies across European respondents in 17 countries. A similar result is found in Legewie (2013), who shows that a terrorist attack in Bali, Indonesia on October 12, 2012

increased anti-immigration attitudes among in Portugal and Poland. Schüller (2016) documents that 9/11 increased anti-immigration attitudes among Germans. Using Eurobarometer survey, Nussio et al. (2019) show that the terrific terrorist attack at the *Bataclan* on November 13, 2015 worsened attitudes toward migrants and refugees without displaying clear geographic patterns across Europe. We know very little, instead, about the local effect of terrorist attacks. Early evidence from France, instead, suggests that focusing on the local effect of terrorist attacks yields null effects on ethnic prejudice (Van Hauwaert & Huber, 2020).

[3]In appendix, I also study the effect of some jihadist episodes that did not cause any death, as well as the effect of common murders perpetrated by Muslim citizens.

[4]Such design is becoming increasingly popular in political science (see Munoz, Falco-Gimeno, & Hernandez, 2018). It was used in different context *e.g.* the effect of terrorism on other outcomes (Geys & Qari, 2017b, Balcells & Torrats-Espinosa, 2018), the effect of electoral outcomes (Giani & Méon, 2017) or sport events (Depetris-Chauvin, Durante, & Campante, 2018) on racial attitudes, or the effect of policy decisions in the context on political attitudes (Bol, Giani, Blais, & Loewen, 2020).

[5]In an extreme case, it could be that there is no overlap between control and treatment group before and after each terrorist attack. The problem would be particularly severe in countries characterized by large regional heterogeneity. This is, however, not the case. The ESS sampling procedure mitigates this potential threat to identification. Interviewers are dispatched to collect interviews simultaneously across different geographic units. As such, the density of survey collection by geographic unit before and after terrorist attacks are similar.

[6]For this specification I drop individuals from Israel due to lack of information.

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A *Fear without Prejudice in the Shadow of Jihadist Threat*

A.1 Descriptive statistics

Table 2 provides the descriptive statistics for the control and treatment group.

A.2 Threats to identification

I discuss here the main threats to identification discussed in the paper.

A.2.1 Potential sample imbalance

Covariates. The irregular daily collection protocol observed in 1 suggests that while the survey is representative of each country for the whole fieldwork period, it may not be so within sub-periods. Treated and control units may then be imbalanced on socioeconomic covariates that correlate with geographic areas. I deal with this issue by matching untreated and treated units within each sampled country on key imbalanced socioeconomic covariates prior to the inference based on the contribution of Iacus, King, & Porro (2012).

Pre-treatment matching requires the following steps. (i) I select a set of covariates so as to balance out the need to account for key information about units with that of avoiding the curse of dimensionality. Only proper variables can be selected.[1] A reasonable choice is to include all basic controls. (ii) I test the whether the distribution of each independent variable is unbalanced between control and treatment group for each terrorist attack. Univariate imbalance does not simply test whether means are significantly different, but also looks at quartiles of the distribution. (iii) Finally, following the application of King et al. (2010), I only match control and treated units on significantly unbalanced covariates.[2] This concludes the *ex ante* control for imbalance.

Table 2 provides information about the mean imbalance between the control and treated group for each variable, providing some by country descriptive statistics. Whereas for dummy variables this corresponds to testing whether means are different, for discrete variables (income and education) or continuous variables (age), I test whether the distribution is significantly imbalanced. After matching individuals on imbalanced covariates (in bold), I prune those observations that turn out to be unmatched. It is according to this strategy, therefore, that I extract outliers. The last four rows provide detailed information about the number of matches among untreated and treated units.[3]

Units of sampling. In Germany, both the control and the treatment group include units from 15 geographic units. The same holds true for Russia (10 geographic units, both in 2008 and 2001). In other cases, some imbalance in regional rate of survey collection remains. In France (2015), the control group includes respondents from 21 regions, whereas the treatment group includes respondents from 18 regions. Similar rate of imbalance characterize the Netherlands (39 geographic unit in the control group and 32 in the treatment group) and Sweden (20 v. 15). While overall regional imbalance is very limited, further effort can be made to rule out this potential level of imbalance. Following Hainmueller (2012), and based on the algorithm presented in Hainmueller & Xu (2013), I weight control units such that the regional distribution of respondents in the control groups matches the one in the treatment group (there is no region sampled in the treatment group and not in the control group). This preprocessing prunes respondents from geographic units for which there are no data in the treatment group, weighting all others according to the share of respondents in each geographic unit in the treatment group. I provide treatment effect with and without entropy balance, showing that they are very close to each other (3).

A.2.2 POTENTIAL DROP IN SURVEY COLLECTION

Data collection in Figure 1 shows that, in some cases, the density of collected surveys falls following the terrorist attack. It is important to discuss whether this happens because of the attack itself, or whether it is due to other, unrelated reasons. Comparing the evolution of data collection after the *Charlie Hebdo* attack in France (2015) and the attack in Berlin (2016) can be helpful to understand what 'other reasons' may include, because the two attacks share similar scale and have been perpetrated in similar countries. In the former case, the rate of collection increases after the terrorist attack, whereas in the second case it decreases. The reason for this, however, has little to do with terrorist attacks. The attack in Paris took place on January 7, after Christmas holidays, whereas the attack in Berlin took place right before Christmas, on the 19th of December.

It must be highlighted that the ESS's sampling strategy is set before the interviews are conducted and does not deviate from this. As stated in the sampling guide of the ESS's website, "Substitution of non-responding households or individuals (whether 'refusals', 'non-contacts' or 'ineligibles') is not permitted at any stage." As such, we can safely exclude the possibility that respondents whose geographic location correlates with particular attitudes, be substituted *ex post* with other respondents. In addition, not only interviewers' main income is largely based on "per completed interview" (In the last wave, the latter is worth about 60 euros), but in addition to that interviewers' contractual arrangements always include bonuses for response rate is over the target rate or, sometimes, for follow-ups on "difficult" cases. Overall, the organization of the sampling and the structure of incentives run against drops in collection effort.

The left panel of Figure 2 aggregates all attacks, focusing on an interval of 120 days before and after the attack. If we restrict the attention to an interval of 15 days after terrorist attacks, we do not observe any drop in collection effort. One can however observe a minor drop in the immediate aftermath of attacks, two days after terrorist attacks. This drop, however, is very unlikely to be caused by terrorist attacks. Indeed, the right panel of Figure 2 focuses on the density of survey collection in the geographic unit where attacks took place. If terrorist attacks were responsible for a decrease in the rate at which surveys are collected, then the drop should be much stronger in targeted area. But this is clearly not the case: in the immediate aftermath of terrorist events, the density of survey collection in the geographic unit where attacks took place is higher than before.

A.2.3 Missing values

One further threat to identification is that, following terrorist attacks, interviewed units are reluctant to answer question pertaining to discriminatory attitudes. This would result in a significantly higher rate of missing data on dependent variables, raising a flag of doubt about the fact that, perhaps, a higher rate of missing data hides discriminatory preferences. Table 4, however, discards this possibility. I test whether the number of missing data for each of the dependent variables significantly change due to the occurrence of terrorist attacks, finding no significant result in either case. I hence conclude that my main results are not affected by a suspicious increase in missing data.

A.2.4 Potential reverse causality

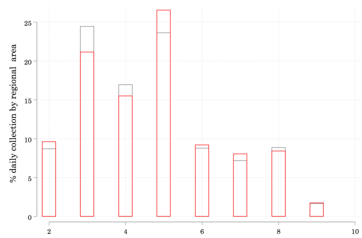
Are terrorist attacks exogenous? The question seems redundant if one focuses on the strategic nature of terrorism that seeks to unexpectedly hit individuals. But the relationship between terrorism and support for ethnic discrimination is more complex than this. The satirical approach of the French magazine *Charlie Hebdo* towards Islam, in fact, had spurred resentment among French Muslims prior to the terrorist attack, possibly altering the attitudes of the French Public prior to the attack.

In sum, whereas the date of the interview is credibly random with respect to the exact date of the attack, the location and broad interval of time chosen by terrorists may not be. Restricting the treatment group to a smaller interval of time (3a) suggests that the timing of the attack is as good as random. Similarly, in Figure 3, I randomly re-assign the treatment within countries and test for "permuted terror treatment effects" on the two main dependent variables. This permutation test strengthens the validity of terrorist attacks as an exogenous shock to security concerns.

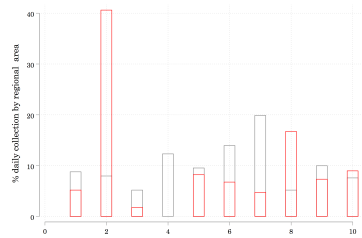
VARIABLES	Control group N	mean	sd	min	max	Treatment group N	mean	sd	min	max
Security fear	10,407	4.734	1.252	1	6	6,387	4.882	1.162	1	6
Ethnic prejudice	10,342	0.620	0.962	-3	3	6,151	0.622	0.994	-3	3
Female	10,814	1.535	0.499	1	2	6,574	1.550	0.498	1	2
Age	10,767	48.14	19.09	15	100	6,540	46.01	18.38	15	100
Household status	10,801	1.625	0.484	1	2	6,569	1.599	0.490	1	2
Education attainment	10,756	4.082	1.828	1	7	6,522	4.334	1.815	1	7
Income decile	9,058	5.448	2.708	1	10	5,254	5.618	2.783	1	10
Domicile	10,807	2.558	1.258	1	5	6,563	2.306	1.218	1	5
Immigration background	10,816	0.364	0.481	0	1	6,574	0.295	0.456	0	1
Employment status	10,816	0.0690	0.253	0	1	6,574	0.0540	0.226	0	1
Living in targeted area	7,677	0.0676	0.251	0	1	4,694	0.153	0.360	0	1
Value of safety	10,465	2.552	1.319	-1	4	6,416	2.697	1.254	-1	4
Value of equality	10,467	2.021	1.065	1	6	6,399	2.133	1.075	1	6
Value of meritocracy	10,396	3.083	1.443	1	6	6,371	2.921	1.382	1	6
Opposition to different race immigration	10,421	2.473	0.944	1	4	6,216	2.594	0.961	1	4
Opposition to same race immigration	10,495	1.850	0.881	1	4	6,282	1.961	0.969	1	4
Opposition to Gypsy immigration	3,467	3.048	0.970	1	4	778	3.126	0.960	1	4
Opposition to Jewish immigration	3,538	1.759	0.910	1	4	808	1.658	0.862	1	4
Opposition to Muslim immigration	3,516	2.931	0.991	1	4	794	3.016	0.983	1	4
Redistribution	10,650	2.036	1.052	1	5	6,466	2.017	1.030	1	5
Left-right placement	9,925	5.235	2.500	1	0	5,407	5.384	2.323	0	10
Government satisfaction	10,535	4.313	2.464	0	10	6,351	4.531	2.468	0	10
Cultural attitudes towards immigration	10,273	5.618	2.620	0	10	6,152	4.898	2.793	0	10
Economic attitudes towards immigration	10,312	5.041	2.535	0	10	6,096	4.619	2.585	0	10
Generic attitudes towards immigration	10,307	5.031	2.411	0	10	6,137	4.412	2.550	0	10

TABLE 1: Descriptive statistics.

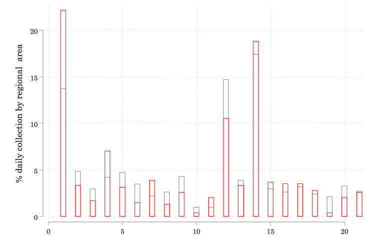
FIGURE 1: Survey collection by region before and after each terrorist attack, by region. The red (gray) histogram refers to the percentage of surveys collected in each region in the control (treatment) group. Information is not available for Israel.



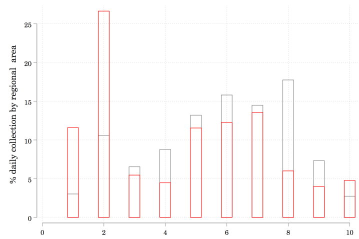
(A) Netherlands, 2004.



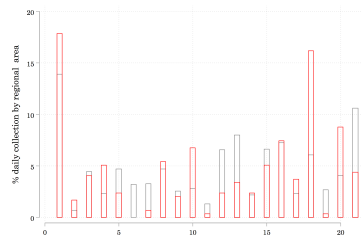
(B) Russia, 2008.



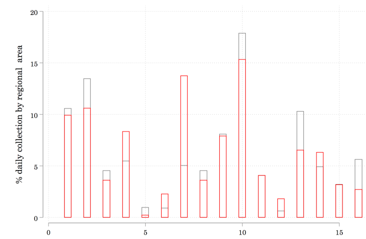
(C) Sweden, 2010.



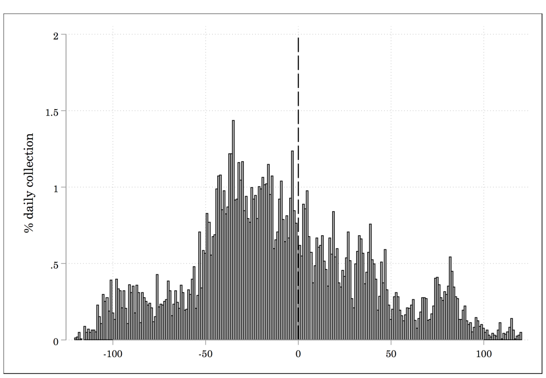
(D) Russia, 2011.



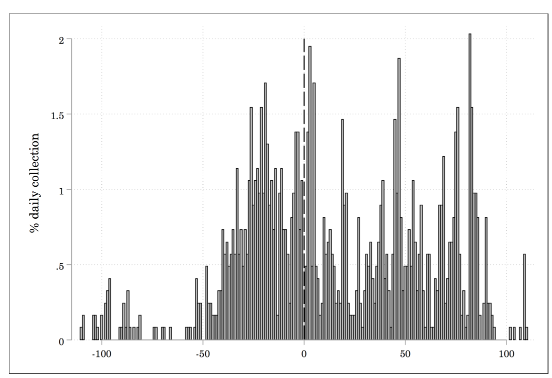
(E) France, 2015.



(F) Germany, 2016.



(A) All areas.



(B) Terror areas only.

FIGURE 2: Density of data collection around terrorist attacks, 120 days before and after.

Country:	IL	NL	RU	SW	RU	IL	FR	IL	DE
Year:	2003	2004	2008	2010	2011	2012	2015	2015	2016
Deaths (injured):	22 (NA)	1 (1)	3 (0)	1 (2)	38 (168)	3 (2)	17 (16)	1 (1)	12 (48)
Income (0-10)	**-.82**	-.09	**.89**	**.19**	**.96**	**.50**	**.27**	.05	.19
Education (0-7)	**-.03**	**.42**	**.26**	**.26**	**.17**	.01	**.45**	.06	**-19**
Age (0-99)	**3.84**	**-.71**	**-.27**	**-4.53**	**-2.52**	**-3.70**	**-2.15**	**1.25**	**-3.44**
Female (0-1)	.04	.06	-.05	-.01	-.05	-.04	.00	-.03	.00
Household status (0-1)	**.06**	.02	-.12	-.08	-.03	-.01	-.01	-.00	-.07
Immigration background (0-1)	-.08	-.04	.02	.08	.01	**-.20**	**.07**	-.03	.04
Employment status (0-1)	.08	-.02	.00	.01	.00	.01	-.00	-.06	.04
Degree of urbanization (0-4)	-.17	-.07	**-.67**	-.15	**.21**	.16	**-.53**	.05	.04
N. untreated	608	1106	252	954	767	1131	1604	2008	2386
Matches	512	1105	250	937	766	1115	1490	2008	2380
N. treated	149	762	683	542	1816	1350	297	530	444
Matches	133	758	583	539	1765	1322	290	530	441

Notes: Each column reports the difference in means between control and treatment group for each single terror attack. Bold numbers indicate that the distribution of a that covariate is significantly different at least at $p < .1$ in at least one quartile of the distribution. Source: *ESS*, rounds 1-8.

TABLE 2: Univariate imbalance.

Date	2003	2004	2008	2010	2011	2012	2015	2015	2016
Country	IL	NL	RU	SW	RU	IL	FR	IL	DE
Perpetrator	Hamas	Hofstad Network	Caucasus Emirate	Al Quaeda	Caucasus Emirate	Hamas	Al Quaeda	Hamas	Isis
Casualties:	22	1	3	1	38	3	17	1	12
					Security fear (0-5)				
Treatment	.211	.235	.078	.250	.268	.102	.222	-.120	-.040
SE	(.127)	(.092)	(.153)	(.152)	(.068)	(.074)	(.115)	(.108)	(.119)
Weighted	NA	.247	-.160	.193	.261	NA	.269	NA	-.027
SE	NA	(.095)	(.178)	(.154)	(.068)	NA	(.111)	NA	(.118)
N. Obs	453	1,064	323	916	898	928	1,561	1,510	2,228
					Ethnic prejudice (0-6)				
Treatment	-.147	.008	-.005	.024	.069	.002	-.119	.065	-.084
SE	(.137)	(.053)	(.105)	(.036)	(.075)	(.091)	(.051)	(.111)	(.049)
Weighted	-.146	.031	.126	.011	.041	NA	-.083	NA	-.083
SE	(.129)	(.049)	(.094)	(.038)	(.074)	NA	(.051)	NA	(.050)
N. Obs	438	1,072	296	926	839	876	1,532	1,562	2,221
Domicile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matching	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Output estimated by OLS. Controls include income decile (1-10),education (1-7), age (15-99), gender (0-1) household status (0-1), migration background (0-1) and employment status (0-1). Standard error in parentheses. Include region fixed effects (not available for Israel). Source: ESS, rounds 1-8.

TABLE 3: Terror treatment effects on security fear and ethnic prejudice, by terrorist incident.

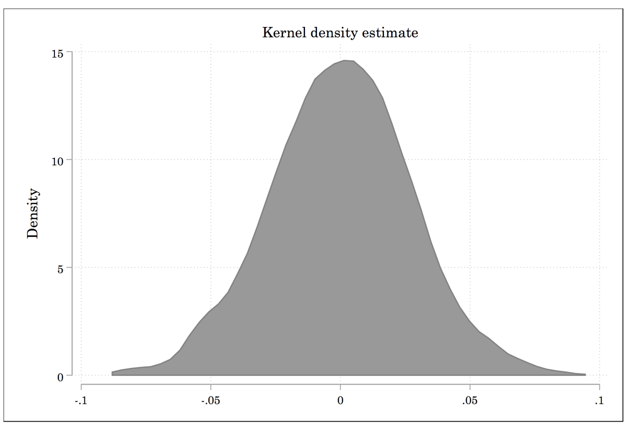
TABLE 4: TERROR TREATMENT EFFECTS ON THE LIKELIHOOD OF MISSING VALUES.

	Security fear				Ethnic prejudice			
	(i)	(ii)	(iii)	(iv)	(i)	(ii)	(iii)	(iv)
Treatment	.00	.00	.00	.00	-.00	-.00	-.00	-.00
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Domicile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matching	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N.obs	10,309	10,309	10,309	10,168	10,309	10,309	10,309	10,168
R-squared	.02	.02	.02	.02	.03	.03	.03	.03

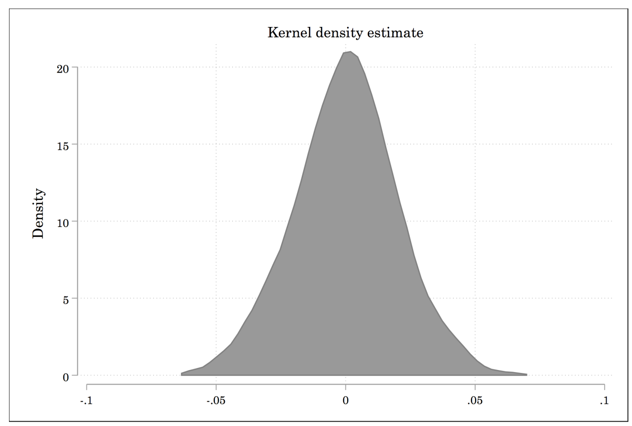
Notes. $^{*}\ p < .10$, $^{**}\ p < .05$, $^{***}\ p < .01$. The reported coefficients are estimated by OLS. Both dependent variables are dummies taking the value 1 if the respondent did not answer the question relative to security fear and ethnic prejudice and 0 else. Robust standard errors (in parentheses) are clustered at individual level. In each regression, the control group is weighted using entropy balancing. Through the latter, the covariates' distribution in the control group mimics the first and second moment of the equivalent distribution in the treatment group. I balance the control and treatment group according to the same variables used as controls in each specification. Each specification includes country-year fixed effects. Domicile fixed effects account for the level of urbanization of the household. The treatment variable is a dummy taking the value 1 if the respondent was interviewed after each of the recorded jihadist attacks, during an interval of 15 days. Controls include income decile (1-10), education (1-7), age (15-99), gender (0-1), household status (0-1), migration background (0-1) and employment status (0-1). Outliers' extraction is carried through Coarsened Exact Matching. Income is coarsened around the median category (5), education is coarsened at university degree (5), age is coarsened in intervals of 10 years within the working age. For each unbalanced dummy variable I apply exact matching. Data include all nine terror attacks as in Figure 1. Source: ESS, rounds 1-8.

FIGURE 3: DENSITY OF PERMUTED TERROR TREATMENT EFFECTS AFTER 10,000 MONTECARLO SIMULATIONS.

(A) Security fear.



(B) Ethnic prejudice.



A.3 Robustness

A.3.1 Full results

In the paper, I focus on terror treatment effects and do not report coefficients and standard errors for controls. Table 5 replicates Table 1 and report coefficients and standard errors for controls as well. This helps profiling the socioeconomic and demographic factors that mostly correlate with security fear and ethnic prejudice.

A.3.2 Time trends

In Table 6 we use the same specification used for Table 1, but further control for a possible trend in discriminatory attitudes, as well as the squared trend to account for possible non-linearities. Treatment effects are slightly larger in absolute magnitude than those obtained with the main specification. Neither the linear nor the quadratic time trend entail a significant effect in most specifications.

A.3.3 Ordered logit

Table 5 replicates Table 1 but accounts for possible non-linearities in the data generating process of the two main dependent variables by running an ordered logit model. Reported odd-ratios indicate qualitatively similar treatment effects.

A.3.4 Clustering by date or country

In Table 9 we replicate the full model in Table 1 by clustering standard errors at date level, finding result that are equal to those in Table 1 until the third decimal. In Table 9 we replicate the full model in Table 1 by clustering standard errors at country level, finding similar values as in Table 1.

A.3.5 Alternative clustering strategy

One important issue is that country-year fixed effects do not fully account for within-cluster correlation or heteroscedasticity. To deal with this potential downward bias in standard errors, one would cluster standard errors at the country-year level. Since, however, the number of clusters is limited (< 30), this approach is likely to underestimate standard errors even if the number of observation per cluster is high, resulting in excess false positives. Therefore, following advice found in Esarey & Menger (2018), I apply a Cluster Adjusted *T-Statistics* (CATs) procedure based on Ibragimov & Müller (2010). The procedure, based on the idea that the data of each cluster can be thought off as a random draw from the total possible observations of the data, is suitable when the number of data in each cluster is relatively large and the number of clusters is low. The logic is quite simple: the model is run separately within every cluster, yielding estimates for the treatment effect in each cluster, then calculating confidence intervals and test statistics using the mean and variance of the collection of cluster-specific coefficient values. As such, standard errors that are robust to clustering even with a very small number of clusters. Simulations by Esarey & Menger (2018) show that this procedure outperforms, compared to other available clustered robust standard error procedures and most notably Pairs cluster bootstrapped t-statistics and Wild cluster bootstrapped t-statistics (both at limiting false positives and at detecting true positives. Table 10 provides terror treatment effects under this alternative specification, showing results close to those found in Table 1.

TABLE 5: MAIN TABLE, INCLUDING CONTROLS.

	Security fear				Ethnic prejudice			
	(i)	(ii)	(iii)	(iv)	(i)	(ii)	(iii)	(iv)
Treatment	.13 (.03)	.16 (.04)	.16 (.04)	.16 (.04)	-.03 (.03)	-.04 (.03)	-.04 (.03)	-.04 (.03)
Age		.00 (.00)	-.00 (.01)	-.00 (.01)		.01 (.00)	-.00 (.00)	-.00 (.00)
Education		-.04 (.01)	-.04 (.01)	-.04 (.01)		-.01 (.01)	-.01 (.01)	-.01 (.01)
Income		-.00 (.01)	-.00 (.01)	.00 (.01)		.02 (.01)	.02 (.01)	.02 (.01)
Female		.05 (.03)	.04 (.03)	.04 (.03)		.08 (.03)	.07 (.03)	.07 (.03)
Household status		-.05 (.04)	-.07 (.04)	-.07 (.04)		.02 (.03)	.01 (.03)	.01 (.03)
Age squared			.00 (.00)	.00 (.00)			.00 (.00)	.00 (.00)
Unemployed			.04 (.07)	.04 (.07)			.09 (.06)	.09 (.06)
Immigration background			.10 (.04)	.10 (.04)			.26 (.04)	.26 (.04)
Constant	3.52 (.06)	3.13 (.16)	3.27 (.21)	3.28 (.21)	3.38 (.03)	2.96 (.12)	3.08 (.16)	3.08 (.16)
Country FE	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Domicile FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Matching	No	No	No	Yes	No	No	No	Yes
N.obs	11,981	10,019	10,019	9,881	11,878	9,900	9,900	9,762
R-squared	.14	.17	.17	.17	.19	.19	.20	.20

Notes. This Table reproduces Table 1 but reports coefficients for each control variable included in the main specification. Source: ESS, rounds 1-8.

TABLE 6: MAIN TEST, INCLUDING TIME TREND.

	Security fear				Ethnic prejudice			
	(i)	(ii)	(iii)	(iv)	(i)	(ii)	(iii)	(iv)
Treatment	0.14 0.16	0.16	0.16	-0.05	-0.06	-0.07	-0.07	
	(0.06)	(0.07)	(0.07)	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)
Trend	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Trend	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
squared	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Country FE	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Domicile FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Matching	No	No	No	Yes	No	No	No	Yes
N.obs	11,981	10,019	10,019	9,881	11,878	9,900	9,900	9,762
R-squared	0.15	0.17	0.17	0.17	0.19	0.19	0.21	0.20

Notes. The reported coefficients are estimated by OLS. Robust standard errors are clustered at country level. The control group in each regression is weighted using entropy balancing on all covariates included in the specification. The covariates' distribution in the control group mimics the first and second moment of the equivalent distribution in the treatment group. Each specification includes country-year fixed effects. Controls include income decile (1-10), education (1-7), age (15-99), gender (0-1), household status (0-1), migration background (0-1) and employment status (0-1). Outliers' extraction is carried through Coarsened Exact Matching. Income is coarsened around the median category (5), education is coarsened at university degree (5), age is coarsened in intervals of 10 years within the working age. For each unbalanced dummy variable I apply exact matching. Data include all nine terror attacks as in Figure 1. Source: ESS, rounds 1-8.

TABLE 7: MAIN TEST, ORDERED LOGIT SPECIFICATION.

	Security fear				Ethnic prejudice			
	(i)	(ii)	(iii)	(iv)	(i)	(ii)	(iii)	(iv)
Treatment	.23	.26	.27	.27	-.09	-.11	-.10	-.10
	(.05)	(.06)	(.06)	(.06)	(.06)	(.07)	(.06)	(.07)
Age		.01	-.01	-.01		.01	-.01	-.01
		(.00)	(.01)	(.01)		(.00)	(.01)	(.01)
Education		-.06	-.06	-.06		-.02	-.01	-.02
		(.02)	(.02)	(.02)		(.02)	(.02)	(.02)
Income		-.00	.00	.00		.05	.05	.05
		(.01)	(.01)	(.01)		(.01)	(.01)	(.01)
Female		.14	.13	.13		.20	.18	.18
		(.06)	(.06)	(.06)		(.07)	(.07)	(.07)
Household status		-.10	-.12	-.12		.07	.03	.04
		(.06)	(.07)	(.07)		(.07)	(.08)	(.08)
Age squared			.00	.00			.00	.00
			(.00)	(.00)			(.00)	(.00)
Unemployed			.08	.09			.12	.13
			(.12)	(.12)			(.14)	(.14)
Immigration background			.17	.17			.55	.55
			(.08)	(.08)			(.08)	(.08)
Country FE	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Domicile FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Matching	No	No	No	Yes	No	No	No	Yes
N.obs	11,981	10,019	10,019	9,881	11,878	9,900	9,900	9,762

Notes. The reported coefficients are treatment effects on security fear and ethnic bias estimated by Ordered logit model. Robust standard errors (in parentheses) are clustered at individual level. In each regression, the control group is weighted using entropy balancing. Through the latter, the covariates' distribution in the control group mimics the first and second moment of the equivalent distribution in the treatment group. I balance the control and treatment group according to the same variables used as controls in each specification. Each specification includes country-year fixed effects. Domicile fixed effects account for the level of urbanization of the household. The treatment variable is a dummy taking the value 1 if the respondent was interviewed after each of the recorded jihadist attacks, during an interval of 15 days. Controls include income decile (1-10), education (1-7), age (15-99), gender (0-1), household status (0-1), migration background (0-1) and employment status (0-1). Outliers' extraction is carried through Coarsened Exact Matching. Income is coarsened around the median category (5), education is coarsened at university degree (5), age is coarsened in intervals of 10 years within the working age. For each unbalanced dummy variable I apply exact matching. Data include all nine terror attacks as in Figure 1. Source: ESS, rounds 1-8.

TABLE 8: MAIN TEST, CLUSTERING (DATE LEVEL).

	Security fear				Ethnic prejudice			
	(i)	(ii)	(iii)	(iv)	(i)	(ii)	(iii)	(iv)
Treatment	0.13	0.16	0.16	0.16	-0.03	-0.04	-0.04	-0.04
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Country FE	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Domicile FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Matching	No	No	No	Yes	No	No	No	Yes
N.obs	11,981	10,019	10,019	9,881	11,878	9,900	9,900	9,762
R-squared	0.14	0.17	0.17	0.17	0.19	0.19	0.20	0.20

Notes. The reported coefficients are estimated by OLS. Robust standard errors are clustered at the date level. The control group in each regression is weighted using entropy balancing on all covariates included in the specification. The covariates' distribution in the control group mimics the first and second moment of the equivalent distribution in the treatment group. Each specification includes country-year fixed effects. Controls include income decile (1-10), education (1-7), age (15-99), gender (0-1), household status (0-1), migration background (0-1) and employment status (0-1). Outliers' extraction is carried through Coarsened Exact Matching. Income is coarsened around the median category (5), education is coarsened at university degree (5), age is coarsened in intervals of 10 years within the working age. For each unbalanced dummy variable I apply exact matching. Data include all nine terror attacks as in Figure 1. Source: ESS, rounds 1-8.

TABLE 9: MAIN TEST, CLUSTERING (STANDARD).

	Security fear				Ethnic prejudice			
	(i)	(ii)	(iii)	(iv)	(i)	(ii)	(iii)	(iv)
Treatment	.13	.16	.16	.16	-.03	-.04	-.04	-.04
	(.06)	(.05)	(.04)	(.04)	(.03)	(.03)	(.03)	(.03)
Country FE	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Domicile FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Matching	No	No	No	Yes	No	No	No	Yes
N.obs	11,981	10,019	10,019	9,881	11,878	9,900	9,900	9,762
R-squared	.14	.17	.17	.17	.19	.19	.20	.20

Notes. The reported coefficients are estimated by OLS. Robust standard errors are clustered at country level. The control group in each regression is weighted using entropy balancing on all covariates included in the specification. The covariates' distribution in the control group mimics the first and second moment of the equivalent distribution in the treatment group. Each specification includes country-year fixed effects. Controls include income decile (1-10), education (1-7), age (15-99), gender (0-1), household status (0-1), migration background (0-1) and employment status (0-1). Outliers' extraction is carried through Coarsened Exact Matching. Income is coarsened around the median category (5), education is coarsened at university degree (5), age is coarsened in intervals of 10 years within the working age. For each unbalanced dummy variable I apply exact matching. Data include all nine terror attacks as in Figure 1. Source: ESS, rounds 1-8.

TABLE 10: MAIN TABLE, ALTERNATIVE CLUSTERING (ADJUSTED T-STATISTICS).

	Security fear				Ethnic prejudice			
	(i)	(ii)	(iii)	(iv)	(i)	(ii)	(iii)	(iv)
Treatment	.11	.13	.12	.13	-.04	-.02	-.02	-.02
	(.05)	(.05)	(.04)	(.05)	(.03)	(.02)	(.03)	(.03)
Country FE	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Domicile FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Matching	No	No	No	Yes	No	No	No	Yes
N.obs	11,981	10,019	10,019	9,881	11,878	9,900	9,900	9,762

Notes. The reported coefficients are estimated by OLS. Robust standard errors are clustered at country level using the Cluster Adjusted T-Statistics procedure. The standard errors returned by the CATs procedure are calculated from the distribution of parameter estimates across the models run in each cluster. These country-level statistics are pooled to produce the reported results. This is part of the reason whjy coefficients are different from those opbserved ion the previous Table where clustering is "standard". Moreover, unlike for previous regression, weighting is not available (errors are bootstrapped). Each specification includes country-year fixed effects. Controls include income decile (1-10), education (1-7), age (15-99), gender (0-1), household status (0-1), migration background (0-1) and employment status (0-1). Outliers' extraction is carried through Coarsened Exact Matching. Income is coarsened around the median category (5), education is coarsened at university degree (5), age is coarsened in intervals of 10 years within the working age. For each unbalanced dummy variable I apply exact matching. Data include all nine terror attacks as in Figure 1. Source: ESS, rounds 1-8.

A.4 Further attitudes

A.4.1 Placebo and alternative proxies for security fear

I replicate the main analysis on three additional survey items. The survey items used are the following:

- Value of Safety: Important to live in safe surroundings. 1: Disagree strongly; ... ; 5: Agree strongly.
- Value of Equality. Important that people are treated equally and receive equal opportunity. 1: Agree strongly; ... ; 5: Disagree strongly.
- Value of Meritocracy: Important to be successful and that people recognize achievements. 1: Agree strongly; ... ; 5: Disagree strongly.

The first item can be seen as a robustness check for the dependent variable security fear, whereas the other two refer to values that are not less obviously impacted by terrorist attacks. Sub-figure 5a confirms that whereas terror treatment effects are positive and significant for value of safety, the placebo-value proxies are left unaffected by terrorist attacks

A.4.2 Placebo preferences

I run a placebo test on policy preferences that are not directly related to terrorism. The survey items used are the following:

- Redistribution: Government should reduce differences in income levels. 1: Agree strongly; ... ; 5: Disagree strongly.
- Left-right placement: In politics people sometimes talk of "left" and "right". Using this card, where would you place yourself on this scale, where 0 means the left and 10 means the right? 1: Left; ... ; 10: Right.

- Government satisfaction: Gays and lesbians should be free to live life as they wish. 1: Very dissatisfied; ... ; 10: Very satisfied.

Sub-figure 5b suggests that my empirical design is not likely to yield spurious results, as none of the tests reject the null hypotheses.

A.5 Fake and failed attacks

I study seven further attacks, limited to European countries, in the same manner.[4] The first category, *fake attacks,* refer to murderous attacks perpetrated by Muslims (or individuals believed to be Muslims in reason of their ethnic background) for which there was, however, no terrorist plot. The second category, *failed attacks,* refer to terrorist plots that caused no victims.

A.5.1 Examples of Terrorist attacks

It is worth giving three examples to clarify at best when an attack qualifies as terrorist attack according to my definition and when it does not:

- *Terrorist attack.* November 4, 2003 (Amsterdam). Mohammed Bouyeri, a prominent member of the *Hofstad group*, a radical Islamic network that *Al Quaeda* repeatedly mentioned in propaganda videos, killed the Dutch film director Theo van Gogh, who had expressed critical views about radical Islam. The attack is recorded in the GTD and falls in the fieldwork period of the second wave of the ESS. This event qualifies as terror attacks according to my definition and can be studied given the survey logistic. It belongs therefore to my case studies.
- *Fake terrorist attack.* January 14, 2003 (Manchester). Kamel Bourgass, an illegal Algerian immigrant and Jihadist sympathizer, killed with a kitchen knife a British police detective during an operation intended to catch illegal immigrants. The attack is not a terrorist attack and hence is not recorded in the GTD,

but does fall in the fieldwork period of the first wave of the ESS. It is recorded, however, in a politicized "terrorist data-set", the thereligionofpeace.com, that includes events of common crime perpetrated by Muslims as "terrorist attacks". I study it as a "fake terrorist attack" (A.5.2).

- *Failed terrorist attack.* September 29, 2010 (Leicestershire). British authorities were alerted by Saudi intelligence that there were parcels carrying explosive devices on board. Four militants were arrested in connection with the attack. Al Qaeda Organization in the Arabian Peninsula claimed responsibility for the attack. While there was a clear terror plot carried out by affiliated Jihadist, the attack did not result in any victim. I study this attack as a "failed terrorist attack" (A.5.3).

A.5.2 Fake attacks

Politicized media treat common murders perpetrated by individuals believed to be Muslims as terrorist attacks. On the overall population, those murderous events should not change either the security fear or the ethnic prejudice. Figure 5 plots the daily distribution for fake terrorist attacks. I found this event in a popular website that lists a large set of alleged jihadist terrorist attacks perpetrated during the last two decades and refer to them as "fake terrorist attack".[5] I include any murderous "fake terrorist attacks" that occurred in the EU available from 2003 to 2016 in the thereligionofpeace.com and not in GTD (last check: August 15, 2017).[6] Tables 11 and 11 show that neither the security fear nor the ethnic prejudice change significantly after each of these attacks.

A.5.3 Failed attacks

Some terrorist plots organized by individuals affiliated with terrorist organization fail, causing no victims. I collect all such cases available in the GTD dataset, check again if they occurred during the fieldwork period of the ESS and analyze them in the same manner as I did for successful attacks. Figure 6 shows where the discontinuity takes place and provides a description of those plots. Tables 13 and 14 show respectively that in none of these cases either the security fear or the ethnic prejudice change significantly.

A.6 Online search

The social desirability of racially unbiased attitudes provides another channel that may explain the documented evolution of immigration-related attitudes around terrorist attacks. Because the ESS is conducted face to face, the respondents may be reluctant to truthfully report their policy preferences, fearing the fact that racially biased attitudes place a social stigma on those who hold them. As such, it may still be the case that the individuals do turn toward racially biased policies following terrorist attacks but do not report this increase in racial bias, implying a downward bias in our estimates. This explanation cannot be entirely ruled out. Yet there are two reasons that limit the explanatory power of this channel.

The first reason is that, as shown in a recent paper by Bursztyn, Egorov, & Fiorin (2017), social norms can sharply change in correspondence with major events, such as the election of Donald Trump. Terrorist attacks may represent similar critical junctures because the racist stances of some opinion leaders tend to legitimate discriminatory attitudes in the public. Moreover, it is unclear whether the report of discriminatory policy attitudes as measured in my dependent variable are truly a taboo. Because the proxy is not particularly intrusive, in the whole control (treatment) group, 39.5% (36.1%) of the respondents self-reported stronger opposition to different race immigration than same-race immigration.

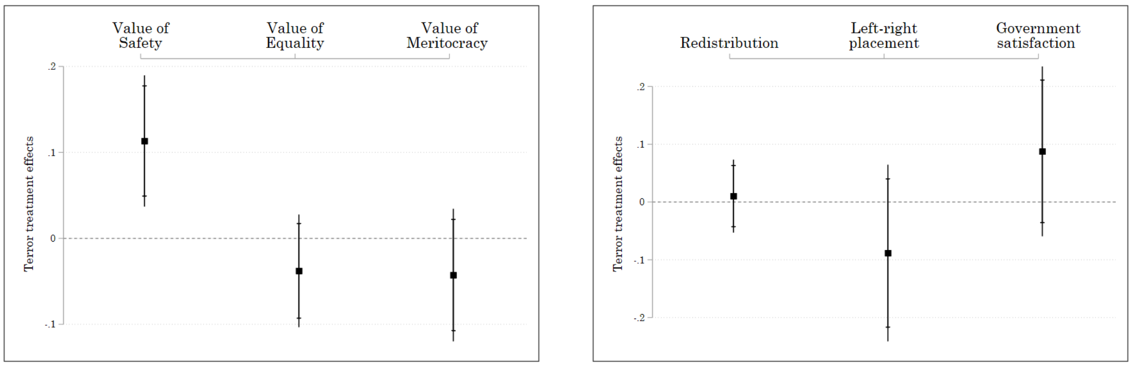
The interviewers may behave differently online when they do not face the perspective of being stigmatized for their attitudes. This is entirely possible. At the aggregate level, though, it does not seem to be overwhelming. Figure 7 reports the google trends for "Immigration", "Terrorism" and "Racism" for the same sample of countries used in the main analysis and within the same interval of time. After downloading google trends for each key word in the relevant space-time corresponding to the fieldwork periods in Figure 1, I average them to obtain a single measure. We observe that searches for "Terrorism" spike in correspondence of terrorist attacks, but with no correlation with respect to search for immigration, which is salient, or racism, which is less so, consistently with the main findings.

A.7 Conditional effects

In this subsection, to study the extent through which socioeconomic variables affects responses in preferences, I interact the treatment with three main socioeconomic variables: gender, age and education. Overall, as Table 15 shows, socioeconomic variables play no decisive role.

I start by looking at the effect of socioeconomic variables in shaping security fear. Whereas gender and education play no role in mitigating individual responses, the treatment effect is significantly weaker among individuals aged more than 65. Table 15 discloses that the terror treatment effect on ethnic prejudice is significant among men.

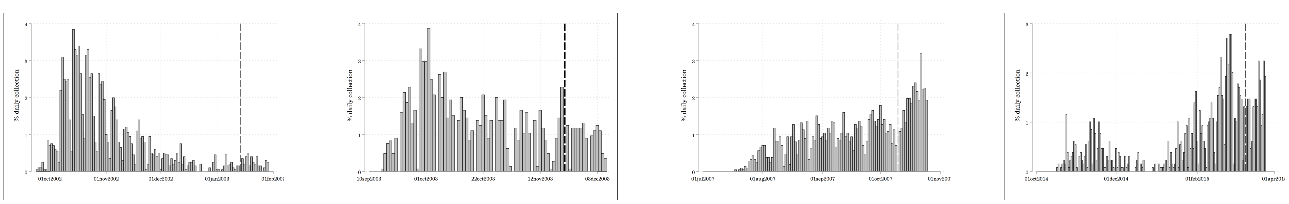
FIGURE 4: TERROR TREATMENT EFFECTS ON FURTHER ATTITUDES.



(A) Value attitudes.

(B) Ideological attitudes.

Notes. Each figure plots terror treatment effects obtained through the same model specification as in Table 1, adding .95 (whole plot) and .90 (capped plot) confidence intervals. The top-right graph is based on data from ESS round 7 only (due to the unavailability of the survey proxy in other surveys). Source: ESS.



(A) U.K., 2003. (Crime against Police) On 14 January, *A police detective and father of three is stabbed to death by a Muslim terrorist*.

(B) France, 2003. (Hate crime) *On 19 November, two Jews are brutally murdered by Muslims in separate attacks in Paris.*

(C) Austria, 2007. (Common murder) On October 10, *In an 'honor attack' a Muslim shoots a man, then cuts off his penis.*

(D) Austria, 2015. (Common murder) On March 10, *A 'migrant' stabs an older man to death on the grounds that his music was against Islam.*

FIGURE 5: Data collection around *fake* terrorist attacks. The description of the event is copied from thereligionofpeace.com.

TABLE 11: SECURITY FEAR, FAKE ATTACKS.

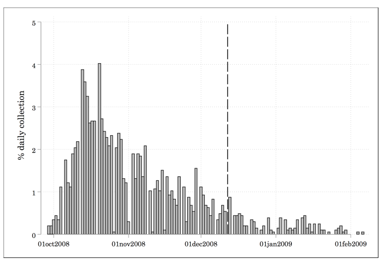
	UK 2003	FR 2003	AT 2007	AT 2015	**ALL**
Treatment	.09	-.05	.07	.02	.02
	(.17)	(.17)	(.07)	(.08)	(.01)
Country FE	.	.	.	.	Yes
Domicile FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Matching	Yes	Yes	Yes	Yes	Yes
N.obs	1,733	1,311	1,255	950	3,938
R-squared	.08	.08	.09	.04	.07

* $p < .1$, ** $p < .05$, *** $p < .01$.

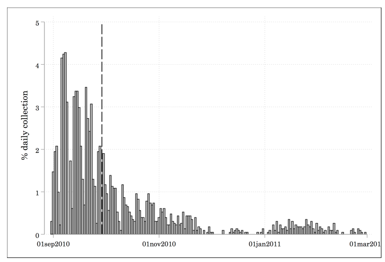
TABLE 12: ETHNIC PREJUDICE, FAKE ATTACKS.

	UK 2003	FR 2003	AT 2007	AT 2015	**ALL**
Treatment	-.06	-.04	-.02	.06	-.00
	(.07)	(.04)	(.04)	(.05)	(.03)
Country FE	.	.	.	.	Yes
Domicile FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Matching	Yes	Yes	Yes	Yes	Yes
N.obs	1,943	1,378	1,240	925	4,108
R-squared	.04	.02	.03	.04	.03

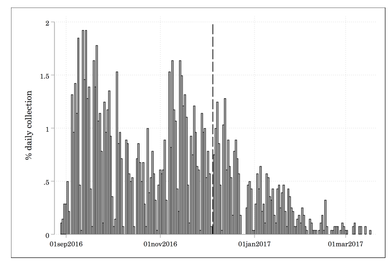
* $p < .1$, ** $p < .05$, *** $p < .01$.



(A) France, 2008. On December 12, a group calling itself the Afghan Revolutionary Front planted a bundle of dynamite in the third-floor restroom of the menswear department inside the Printemps department store in Paris, Ile-de-France, France. The group sent a letter to police saying several bombs were planted in the department store and that they demanded that France withdraw from Afghanistan.



(B) U.K., 2010. On September 29, in East Midlands Airport in Lockington, Leicestershire, Great Britain, British authorities were alerted by Saudi intelligence that there were parcels carrying explosive devices on board. Four militants were arrested in connection with the attack. Al Qaeda Organization in the Arabian Peninsula claimed responsibility for the attack.



(C) Germany, 2016. On December 5 an explosive device was discovered and defused at a Christmas market in Ludwigshafen, Germany. The assailant as a 12-year-old child and posited that he had been inspired by the Islamic State of Iraq and the Levant (ISIL). ISIL then claimed responsibility for the attack.

FIGURE 6: Data collection around *failed* terrorist attacks.

TABLE 13: SECURITY FEAR, FAILED ATTACKS.

	FR 2008	UK 2010	DE 2016	**ALL**
Treatment	-.01	.01	0.06	0.03
	(.17)	(.08)	(.08)	(.06)
Country FE	.	.	.	Yes
Domicile FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Matching	Yes	Yes	Yes	Yes
N.obs	1,591	1,013	2,123	4,727
R-squared	.09	.03	.07	.04

* $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 14: ETHNIC PREJUDICE, FAILED ATTACKS.

	FR 2008	UK 2010	DE 2016	**ALL**
Treatment	-.06	.01	-.06	-.02
	(.07)	(.04)	(.04)	(.03)
Country FE	.	.	.	Yes
Domicile FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Matching	Yes	Yes	Yes	Yes
N.obs	1,730	984	2,116	4,668
R-squared	.04	.04	.03	.05

* $p < .1$, ** $p < .05$, *** $p < .01$.

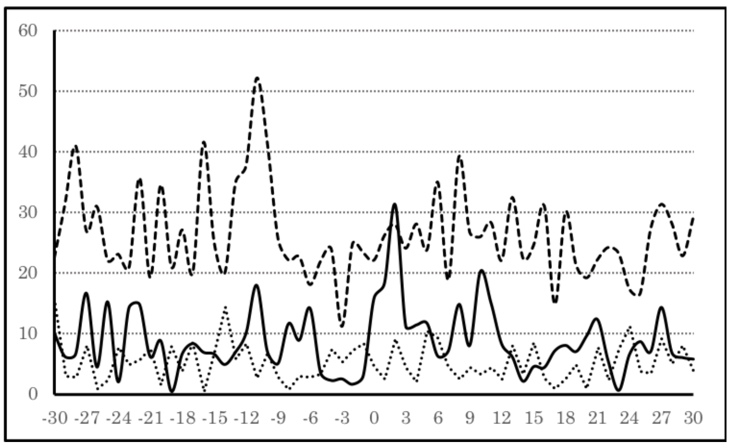


FIGURE 7: Online correlation search for "Immigration" (dashed line), "Terrorism" (regular line) and "Racism" (dotted line). In the $x-$axis, time interval around each terrorist attacks (30 days before and after each attack). In the $y-$axis, average volume of google search for each topic across terrorist episodes. Units in the $y-$axis use information on search traffic on Google browser to compute means relative to an arbitrary initial value with respect to which each data point is scaled. (Israel 2003 is excluded since Google trends started in 2004).

TABLE 15: MAIN TEST, SOCIOECONOMIC ASPECTS.

	Security fear			Ethnic prejudice		
	(i)	(ii)	(iii)	(i)	(ii)	(iii)
Treatment	.28	.36	.10	- .20	-.07	-.04
	(.11)	(.10)	(.04)	(.08)	(.09)	(.03)
Treatment × Female	- .08			.10		
	(.07)			(.06)		
Treatment × Age		-.01			.00	
		(.00)			(.01)	
Treatment × Education			.01			- .05
			(.18)			(.05)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Domicile FE	No	Yes	Yes	Yes	Yes	Yes
Matching	Yes	Yes	Yes	Yes	Yes	Yes
N.obs	9,881	9,881	9,881	9,762	9,762	9,762
R-squared	.17	.17	.17	.20	.20	.20

Notes. This Table reproduces Table 1 but reports coefficients for each control variable included in the main specification and adds interactions with main SES covariates. Source: ESS, rounds 1-8.

A.8 A FORMAL MODEL OF PARTICIPATION TO JIHADIST ACTIVITY

In the paper, I propose a simple loss function that captures in the most parsimonious way a tradeoff implied by an ethnic bias in immigration rules. The latter is micro-founded here through a simple model.

There are a Government G that chooses the immigration policy to minimize terrorist activity, a jihadist organization J that recruits out-group individuals to maximize it, and a positive mass m of out-group individuals that solve a leisure vs. terrorist activity tradeoff.

Without incurring any cost, the government chooses $y \in (-1, 1)$, which captures the immigration policy content as follows:

- positive values indicating an "ethnic bias in immigration rules";
- 0 indicating "neutral immigration rules";
- negative values indicating "affirmative action in immigration rules".

After observing this decision, the terrorist organization sets an income w to affiliate jihadists, and individuals choose their terrorist supply.

Setup. I assume that an ethnic bias in immigration policies, by foreclosing out-group individuals, reduces the pool of potential terrorists. Out-group individuals tradeoff terrorist activity and leisure. In this parsimonious model, an ethnic bias acts a linear tax on leisure and hence reduces the opportunity cost of terrorist activity. Under quasilinear preferences, this increases unambiguously the intensive margin of terrorist activity.

There is a mass of m of out-group individuals. An ethnic bias in immigration policy - by allowing in-group individuals while foreclosing out-group ones - reduces the mass of out-group to $m(1-y)$.[7]

Each out-group individual obtains an exogenous income normalized to 1 from the regular labour market, which I do not model here. Individual i can participate to the jihadist activity. His benefit depends on terrorist participation p and leisure ℓ according to the following quasilinear specification:

$$u_i(p,\ell) = p + (1-y)\ln(\ell). \tag{A.1}$$

The time constraint is given by

$$h + \ell \leq \theta_i + 1,$$

where θ_i is the type of i and is drawn from a uniform distribution $\mathcal{U} \in (0,1)$. Higher values indicate that i has a larger amount of free time, and in equilibrium imply higher terrorist activity. This would indicate that, for instance, unemployed individuals are *ceteris paribus* more likely to join jihadist organization.

In A.1, an ethnic bias in immigration rules acts as a linear tax (or subsidy, if negative) on the leisure of out-group individuals. As such, from the perspective of i, it reduces (or increases) the opportunity cost of terrorist activity. This assumption seeks to capture in the most parsimonious way the idea pioneered by Bueno de Mesquita & Dickson (2007) that lowering the welfare of out-group individuals results in a higher likelihood to turn them into the terrorist activity.

I assume that a jihadist organization maximizes terrorist activity T according to a linear production function $T(k,H) = kH$, where $k \in (0,1)$ is an exogenous linear productivity parameter and H is the total number of hours of terrorist activity supplied by out-group individuals. The jihadist organization maximizes

$$u^J(k,H) = (k-w)H, \tag{A.2}$$

where w is the hourly benefit granted to affiliated jihadists. The latter can be thought to be an income as well as non-material benefits. Notice that the chosen immigration rule does not enter directly the utility function of terrorists. It affects, however, the jihadist organization's recruitment process.

The objective of the government is to minimize terrorist threat T. Utility can be written as

$$u^G(k, H) = -(k - w) H. \quad \text{(A.3)}$$

Hence the government - and our rational, aligned, policy instrumental survey respondent, minimizes the jihadist threat by solving

$$y \text{ argmin } u^J(k, H).$$

To break ties, I assume that in case of indifference between y' and y, $y' \succeq y$ if $y' \geq y$. This choice accounts for the fact that, while policy concerns are priority, the government would *ceteris paribus* prefer to restrict the immigration of out-group individuals, possibly due to cultural or re-election concerns.

Equilibrium. In stage 2, the jihadist organization maximizes terrorist activity. First order condition of A.2 implies $w = k$. Denoting by $\bar{h}$ the average terrorist activity supplied by jihadists and by n the total number of affiliated jihadists, we can write the total terrorist activity as $H = \bar{h} \times n$. Hence, the jihadist organization is indifferent between the extensive and intensive margin of terrorist activity.

We now determine the optimal supply of terrorist activity. Denoting by h the number of hours spent on terrorist activity, the budget constraint can be written as $c = wh$, where $w = k$ is the benefit received from the jihadist organization. As such, taking

y, k and θ_i as given, i solves

$$\max_{h} u_i (c, \ell) = c + (1 - y) \ln (\ell)$$

$$\text{s.t } h = \theta_i + 1 - \ell.$$

The first order condition allows us to write down the intensive margin of the terrorist activity:[8]

$$h_i = 1 + \theta_i - \frac{1-y}{k}. \tag{A.4}$$

If the interior solution A.4 holds, then an increase in y yields

$$\frac{\partial h_i}{\partial y} = \frac{1}{k} > 0, \tag{A.5}$$

and hence, for any value of $y \in (-1, 1)$, *an ethnic bias immigration rules increases the intensive margin of terrorist activity.*

From A.4, out-group i optimally sets $h_i (k, y, \gamma_i) = 0$ if $\theta_i \leq \frac{1-y-k}{k}$. The probability that i "affiliates", *i.e.* participates even minimally to the terrorist activity, is the *extensive margin* of the terrorist activity $n (y)$:

$$n (y) = \left(1 - \int^{\hat{\theta}} \theta_i \, dF (\theta)\right) m (1 - y) = \left(2 - \frac{1-y}{k}\right) m (1 - y). \tag{A.6}$$

Differentiating A.6 with respect to y, we obtain

$$\frac{\partial n (y)}{\partial y} = 2m \left(\frac{1-y-k}{k}\right)$$

and so the condition

$$y \geq 1 - k \tag{A.7}$$

is sufficient so that a *an ethnic bias in immigration rules decreases the extensive margin of terrorist activity.*

In stage 1, the government takes h and k as given and minimizes terrorist activity. The total level of terrorist activity, given A.5 and A.7, can be written as

$$H = \frac{m(1-y)}{2}\left(\frac{2k+y-1}{k}\right)^2.$$

First order condition reveals that H has a maximum at $y^*_{\text{max}} = 1 - \frac{2}{3}k$. Since $k \in (0,1)$, it is always under a racially-biased policy that terrorist activity is the highest.

The optimal immigration policy of the government requires setting

$$y^*_{\text{min}} = 1 - 2k. \tag{A.8}$$

Hence, the minimization problem of the government has a unique solution.[9] Importantly, A.8 can be either positive or negative depending on the level of productivity of the organization.[10] The more productive the jihadist organization, the more important the intensive margin becomes relative to the extensive margin. Consistently, if we think of a jihadist attack as a signal that the organization is becoming more productive, then the optimal policy adjustment requires reducing the ethnic bias in immigration rules: $\frac{\partial y^*_{\text{min}}}{\partial k} < 0$.

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